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BACILLUS TUBERCULOSIS. CONCLUSIONS FROM THE RESULTS OF INVESTIGATION. *By H. D. SCHMIDT, M. D., of New Orleans, La.*

In reviewing the results of my investigations into the nature of the bacterium tuberculosis and its relation to the human lungs, or other organs, it will be found that, excepting the development of this organism in the nuclei of the tubercle-cells, they correspond, perhaps, in most other points to the statements previously made by other authors. The development of the bacterium tuberculosis in, or, as it almost appears, from the protoplasm of these nuclei is a phenomenon, not only difficult to explain, but which, moreover, may yet bear importantly upon the whole bacterium tuberculosis question. Hence it is worthy to be presented to the reader in its proper light, which, however, involves a review of other obscure points in the history of the bacterium tuberculosis, especially of the manner in which this organism is said to gain access to the human lungs, or other organs.

When Koch, in 1882, announced his discovery of the bacillus tuberculosis, he asserted it to represent the original and special cause of tuberculosis, demonstrating, at the same time, the manner in which it had gained access to the human lungs as to give rise to the tubercle of tuberculosis. His explanation was mainly based upon the supposition, that these special bacteria were constantly floating in the air; and that, accordingly, some, or even one, of them would enter the human lungs during the act of inspiration; and, being carried by the inspiratory current of air to the terminations of the bronchial system, the air-vesicles, settle there upon one or the other epithelial cell, lining these minute cavities; the irritation, set up in the protoplasm of this cell by the presence of the parasite, was to be regarded as the first impulse given to the formation of the tubercle.

It is, particularly, the giant-cells of the tubercle which Koch regards as the special seat of the infectious agent; *for he says, as soon as they make their appearance in the tubercles, bacilli tuberculosis will be regularly met with; if the number of the latter in the tubercle is very limited, they will almost always be found in the giant-cells; and, then, never more than one, or a few individuals in each cell. But when, in correspondence to a more intense course of the process, the bacilli are present in large numbers, then many, fifty or more, will be found enclosed in one giant-cell. The arrangement of these organisms in the giant-cells, Koch states, often takes place in a peculiar manner, namely: If there is only one bacillus enclosed in a cell, it will mostly be found in the centre of the latter, or very little excentrically; but, if the nuclei are lying closely together near, or at one end or pole of the cell, then the bacillus will be found at

* "Die Aetiologie der Tuberculose," von Dr. R. Koch, reviewed by Litten. "Deutsche Medicinal Zeitung," March 24, 1884.

IN the month of March, 1885, Dr. JAMES NEVINS HYDE, Professor of Diseases of the Skin in Rush Medical College, Chicago, published a paper in the *Chicago Medical Journal and Examiner* entitled "On the Affections of the Skin, Induced by Temperature Variations in Cold Weather." This was followed, in February, 1886, by a second paper, more fully considering the same subject, including the treatment of the so-called "Prairie Itch," "Ohio Scratches," etc.

As both of these papers were soon out of print, the publishers have reproduced the two in a single monograph, with index, which the author has carefully revised in every part, and to which he has made a large number of additions, relating more particularly to the treatment of the class of disorders under discussion.

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the other end, that is, in that part of the cell which is free from nuclei. This arrangement of the nuclei opposite to the bacillus, Koch regards as a manifestation of an "antagonism," existing between the nuclei of the giant-cell and the parasite therein enclosed, causing the former to get as far as possible away from the latter; even if the bacilli in the giant-cell increase in number, this "oppositional" grouping of the nuclei may still take place, though then, an entirely different arrangement of the bacilli is generally observed. To Koch it almost appears that with the numerical increase of the bacilli, their attitude (*Haltung*) or bearing toward the nuclei becomes more active; for, he says, they then advance more and more toward the periphery of the cells; and, inserting themselves between the nuclei, finally even break through the walls of these bodies. Besides, the destruction of the giant-cell appears to follow regularly such an increase of the bacilli; for very often, in the vicinity of giant-cells containing bacilli arranged in a radiating manner, and in a direction toward the interior of the tuberculous centre, groups of bacilli are met with, which, though not any more enclosed in brown-colored nuclei, still show the same radiating arrangement. As many transitive forms are besides met with, it cannot be doubted that such radiating groups of bacilli indicate the places of former giant-cells, of which the nuclei have disappeared, and of the contents of which only the bacilli have been left.

The above mentioned statements of Koch show that he attaches much importance to the relationship existing between the bacteria tuberculosis and the giant cells. As far as I am able to interpret these statements, he regards the appearance of these giant-cells and that of the tubercle-bacteria as occurring almost simultaneously, though it is not stated whether the bacterium gave rise to the giant-cell, or whether the latter pro-

duced the former. If the number of bacteria is very limited, Koch finds them always confined to the giant-cells, but never more than one, or a few individuals in each cell. From this I cannot but infer, that he regards these individuals as the original bacteria, carried by the inspiratory current of air to the interior of the air-vesicles of the lung, where, by irritating the protoplasm of one or the other lining epithelial cells, they give rise to the formation of a giant-cell. Although the exact nature of these giant-cells is as yet not positively known, we shall in this instance, nevertheless, suppose that they represent irritated epithelial cells, the nuclei of which have multiplied by division, while the protoplasm of the cells has simply increased in bulk without division. The inference to be drawn from Koch's observations on the giant-cells would be as follows: The bacterium, after having caused by its presence the multiplication of the original nucleus and the increase of the protoplasm of the cell, quietly remains in one corner of the latter, while the nuclei, being, as it appears, afraid of the parasite, get away as far as possible from their antagonist by withdrawing to the other corner. But, now, the bacterium also commences to increase in number by reproducing its own kind, and, as soon as its descendants are sufficiently numerous, their attitude toward the nuclei becomes more active; for, advancing like so many soldiers, toward the periphery of the cell, they commence the attack upon the nuclei by inserting themselves between these bodies, and finally conquer by breaking through their walls and by settling in their interior. Thus the "antagonism," which Koch supposes to exist between the bacteria and the nuclei of the giant-cell, leads to the destruction of the latter, and to the remaining of the former upon the field of battle.

As far as my own observations extend I may state that I

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have never observed the above described phenomenon concerning the bacteria tuberculosis and the nuclei of the giant-cells, which, if true, would almost indicate that these bodies were endowed with a certain kind of instinct, manifesting itself in the "antagonism" to which Koch directs attention. On the contrary, I have met with many giant-cells containing no bacteria tuberculosis, whilst in others I found a smaller or greater number of these organisms enclosed, both in the nuclei and in the protoplasm of the cells. In the same way giant-cells are met with containing a considerable number of nuclei, arranged in line along a part of the periphery of the cell, whilst in others again they are, in smaller or larger numbers, irregularly distributed throughout the protoplasm of the cell. On the whole, I can find no reason for supposing a closer relationship to exist between the giant-cells and the bacteria tuberculosis than between the latter and the other cells of the tubercle. In those instances in which I met with bacteria tuberculosis in the giant-cells I found them either still enclosed in the nuclei or liberated from the latter by the ultimate disintegration of their protoplasm, lying free in the degenerated protoplasm of the cell. But let us now endeavor to find some plausible explanation for the presence of the bacteria tuberculosis in the nuclei of the tubercle cells, in which, as I have shown before, they make their first appearance, and to which they may gain access by different means and ways. Before, however, entering upon this discussion I shall take the liberty of quoting from Litten's review, already mentioned, Koch's views on the subject. They are as follows: "The appearance of one or some bacilli in the interior of cells, bearing an epithelioid character, must be regarded as the first stage of the origin of the tubercle. The manner in which the bacilli get into these cells can hardly be explained in any

other way but that, not being themselves endowed with spontaneous motion, they are either in the current of the blood or lymph or in the tissues of already existing tuberculous centres, taken up and carried along by tissue-elements which possess this kind of motion, as, for instance, the wandering cells. The peculiar fact that single bacilli, or small groups of them, separated from each other by one and the same, though comparatively far, distance, are frequently met with, can only be explained in this manner. Charged with such parasites, the cell is still capable of traveling considerable distances, but under the deleterious influence of the bacillus it undergoes changes which will soon cause it to stop. Whether, now, one wandering cell is destroyed and the bacilli are taken up by other cells present at the same place, and which subsequently assume an epithelioid character,—or whether, what Koch thinks to be more probable, the wandering cell, transporting the bacillus is transformed into an epithelioid cell and from this into a giant-cell must, for the present, remain undecided. In order to presume that the bacilli are originally carried along by the wandering cells and that in this manner their distribution in the tissues is effected, the following reasons may be adduced. First, the analogy with the septicæmia of the mice, in which also rod-shaped bacilli are incorporated by colorless blood-corpuscles; furthermore, the direct observation, showing that the tubercle bacilli are first taken up and transported by the wandering cells. This is best recognized in those cases in which large numbers of bacilli are introduced into the circulation, as, for instance, by injection into the veins of the ear of the rabbit. If an animal, infected in this manner, is killed at an early time, numerous leucocytes enclosing one or some bacilli are then still found in the blood; whilst even in the tissues of the lung, liver and spleen true round cells now and

then appear which, containing a single or divided nucleus, show as yet no epithelial form; that is to say, they resemble in every respect colorless blood-corpuscles, though containing tubercle bacilli. The same phenomena were observed in guinea-pigs, into the abdominal cavity of which large numbers of tubercle bacilli were injected, and which died already during the course of the first week. A third reason for the above mentioned supposition may be found in completely dead tissues, that is, in such places where the influence of living cells upon the bacilli is entirely excluded; for if here a vigorous growth of bacilli once more occurs, they will arrange themselves in entirely typically formed groups resembling the peculiar forms of bacteria-colonies in the pure culture upon the serum of blood. These forms have to be regarded as those assumed by the tubercle bacilli when developing without disturbance, and when their grouping is solely determined by those local changes of position which depend upon their growth. Every other arrangement must be regarded as caused by some other disturbance. If a too rapid increase of the bacilli within the giant-cells takes place, the latter undergo destruction in being to a certain extent burst by the bacilli pressing themselves between the walls of the nuclei. The nuclei disintegrate into minute granules, and the cell is destroyed. The further changes, taking place in the tuberculous tissues after the development of the epithelioid and the giant cells, are all of a retrograde nature. The greater part of them belongs to those processes known as coagulation-necrosis, and lead to the death of the tuberculous tissues, and to the formation of the so-called cheesy masses which are so frequently found in tuberculous centres. Generally the tubercle-bacilli disappear very soon in the cheesy masses, so that they are only met with in the younger centres (Herden), while they are almost always absent

in the older ones. In other cases, however, a simple shriveling and transformation of the tuberculous tissue into a firm connective tissue may take place after the disappearance of the bacillus-vegetation."*

Now let us briefly review the above quotation from which we gather that the appearance of one, or some bacteria tuberculosis in the interior of cells bearing an epithelioid character, is regarded by Koch "as the first stage of the origin of the tubercle," and that, furthermore, the manner in which the bacteria get into these cells can hardly be explained in any other way but that they themselves, being incapable of moving, must be taken up—either in the current of blood or lymph, or in the tissues of already existing tuberculous centres—and carried along by other tissue elements which possess spontaneous motion, such as the wandering cells. The conjecture concerning the transportation of the bacteria tuberculosis by means of wandering cells, appears to me very plausible, especially since I have observed the same phenomenon in the blood of lepers, where I found the bacteria lepræ adhering to the remains of colorless blood corpuscles. But with this conjecture the presence of these organisms in the tubercles of tuberculosis, and particularly in the nuclei of the tubercle cells, is as yet not explained, even not in accepting the theory of the tubercle being originally formed from wandering cells, derived from the blood or lymph, for it is obvious that the bacteria, in order to be taken up by these cells, must already be present either in these fluids, or in already existing tubercles. But as to the particular manner in which the bacteria first gain access to these fluids, or to the parenchyma of the lungs, nothing is said in this place; and, as no other satisfactory explanation on this point can be found in Litten's review

* Page 261.

of Koch's paper, I propose to discuss the different ways and manners in which the bacteria might possibly get into the tissues of the human lungs, or other organs.

The bacteria tuberculosis, being incapable of moving spontaneously, must of course be transported from one place to another by some other agent or moving power. If, therefore, they are, as Koch asserts, constantly present in the air we breathe, it is but natural to suppose that one, or some of them might be carried, as once before mentioned, with the inspiratory current of air into the interior of the alveolar cavities of the lungs, and settle there upon some of the lining epithelial cells. Having settled upon one of these cells, however, the bacterium being motionless, is incapable of penetrating through the protoplasm of the cell into the nucleus, except by decomposing and absorbing the protoplasm before it. This phenomenon is observed among some of the more highly organized fungi, as, for instance, with the so-called potato-fungus (*Peronospora infestans*), which, though being, like the bacterium tuberculosis, incapable of moving spontaneously, nevertheless penetrates not only into the cells of the potato, but, moreover, even into the starch-granules contained therein. The penetration in this case, however, is solely effected by the inherent growth of the fungus, that is, by the lengthening of its filaments, which, in channeling their way into the interior of the cell, absorb a part of its wall. Thus, the further the fungus penetrates, the more its filaments gain in length.

Although it is not very probable, I will, for the sake of argument, suppose that the bacterium tuberculosis, after having settled upon the epithelial cell penetrates in the above-mentioned manner into the interior of the nucleus, whilst it, at the same time, increases in numbers; in this way it might even penetrate from cell to cell. But instead of proceeding with

our supposition, let us now direct our attention to the tubercle, which, according to Koch, has to originate in the epithelial cell, irritated and already partly destroyed by the bacteria. The tubercle, as we know, can only be formed by the multiplication of one or more cells, brought about by the successive division of the nucleus and the protoplasm of the original cell. The question, therefore, arises: Is a cell, which is already partly destroyed by bacteria, still capable of multiplying; or does it still possess sufficient vitality to give rise to the hundreds or thousands of cells of which a tubercle is formed? For my own part, I cannot but answer the question in the negative. But in admitting even the possibility of the tubercle arising from an epithelial cell, already crippled by bacteria, there are besides other phenomena remaining to be satisfactorily explained. One of these is the simultaneous origin of great numbers of tubercles throughout one, or even both lungs, as is frequently witnessed in acute miliary tuberculosis. In such a case, it is quite difficult to suppose that so many bacteria, as there are tubercles, had simultaneously entered the lungs, and that—while passing through the numerous bronchi and bronchioles—they should distribute themselves so nicely as to settle only in the interior of certain alveoli throughout the whole organ, separated from one another by certain, though frequently quite small, distances, as found between the respective tubercles supposed to be called forth by these bacteria. The appearance of tubercles, separated from one another by greater or lesser distances, has been explained by supposing that the bacteria are transported from one part of the lung to the other by means of the expectoration; that is to say, by the matter derived from a small cavity, and charged with bacteria, passing outward through one of the smaller bronchial tubes into the larger parent tube, and then returning through another

branch of the latter to the terminal lobules of another part of the organ. In answer to this supposition we may say, that it is possible but not very probable, as the normal direction of the secretion is from the smaller bronchi toward the larger ones, the same as that of the movements of the cilia of the epithelial cells lining the bronchi, and assisting in the forward movement of the secretion. But the main objection to this supposition will be found in the fact that not unfrequently cases of miliary tuberculosis are met with in which no cavity whatever can be discovered, though innumerable tubercles are disseminated throughout the whole organ.

There is another mode of explaining the transportation of bacteria from one tubercle to a distant part of the lung; it is the same as that used to explain the phenomenon of metastasis observed in certain morbid growths, that is, through the lymphatics. The fact that all tissues contain lymphatic channels or vessels, makes it appear very plausible that certain neoplastic cells might, in one way or the other, get into these channels to be carried by the lymph-current to other parts of the same, or even of another organ. Such an occurrence, it is true, may happen, but not as easily as is generally supposed.

The lymphatics commence, as we know, in the so-called lymph-spaces, representing minute, irregularly-shaped channels extending, especially through the connective tissues, where they are identical with the interspaces of the fibrillar bundles of this tissue and lined, though incompletely, by endothelial cells. In these channels the radicals of the lymphatic vessels commence. Each organ generally possesses a deep and a superficial set of lymphatic vessels. The radicals of the deep lymphatics, after having arisen from the lymph-channels, generally follow the minute bloodvessels, and, soon joining one another, form larger lymphatic vessels. By successively continued junctures of the

latter, the larger lymphatic vessels are formed, which, also following the bloodvessels, finally leave the organ to join the nearest lymphatic glands. But while the radicals of the deep lymphatics, after arising from the lymph-spaces, soon join one another, to give rise to larger lymphatic vessels, those of the superficial lymphatics first form a network which extends over the surface of the organ, and from which the lymphatic vessels subsequently arise. The lymphatic vessels, leaving the lymphatic glands, form larger vessels which finally join one or the other lymphatic duct. Some communications always exist between the deep and the superficial lymphatic vessels of an organ. All lymphatic vessels are provided with valves, which open in the direction of the lymph current, that is, toward the opening of the lymphatic ducts into the subclavian veins, or, in other words, toward the heart. Even the radicals of the lymphatic vessels possess these valves, the presence of which is indicated by a constriction in the wall of the vessel. Many years ago, I injected the lymphatics of the human liver with Prussian blue and even observed these constrictions on lymphatic vessels, smaller in diameter than the capillaries of the blood. From this we may presume, that the current of lymph can only run in one direction. In the lymphatic network on the surface of an organ the current may run sideways or backward into neighboring radicals, but only to a very small extent, as even here, its general direction is toward the lymphatic vessels arising from this network. The lymph-current, joining the blood-current in the subclavian veins, runs of course in the same direction with this, and is, therefore, controlled and kept in motion by the same power, upon which the circulation of the blood depends, that is, the suction-power of the heart. Hence we may presume that the lymph-current, from its very beginning in the lymph-spaces, cannot deviate from its

course, but is bound to take the most direct course toward the heart. For this reason we are justified to presume that, if ever bacteria tuberculosis are taken up by the radicals of the lymphatics of the lung, they will be carried by the lymph-current directly to the heart, and not to a distant terminal lobule of this organ, except through the medium of the circulation of the blood. Having once entered the blood, the bacteria might attach themselves to some of the colorless blood-corpuscles, and pass with these through the walls of the capillaries of one or the other organ, and thus give rise to the formation of new tubercles.

The formation of tubercles in this manner is not impossible, though quite improbable; for if this were the case, bacteria tuberculosis would much more frequently be met with in the blood of tuberculous patients than they have in reality been met with thus far. To the extent of my knowledge, there are only three authenticated cases, reported by Weichselbaum, in which these bacteria were found in the blood after death. In these cases cheesy tubercles were found in the uterine veins and Fallopian tubes, in the pulmonary artery, and around the pudendo-vesical plexus, where they had perforated a vein. In ten other cases Weigert has observed tubercles in the veins, and in two others tubercles in the thoracic duct; while tubercles in the latter locality had been previously observed in one case by Ponfick. In the reports of Weigert's and Ponfick's cases, however, nothing is said of the presence of bacteria tuberculosis in the blood, though, if they had really been present in this fluid, it would be obvious that they had entered it directly with the cheesy matter of the tubercles, and not by way of the lymphatics. Nor can the presence of tubercles in the walls of the veins be regarded as an unusual phenomenon, for they are equally met with in the walls of other vessels, or

in the peritoneum and other fibrous structures. I have myself stained the blood of several tuberculous patients by Ehrlich's and Gibbes' methods, but failed to discover any bacteria tuberculosis.

From what I have said above it will be seen, that the theory of the formation of tubercles in the human lungs from one, or several of the lining epithelial cells of the alveoli, previously irritated and crippled by bacteria tuberculosis, is rather resting upon a weak foundation, for if the tubercle was really originating in one or more of these cells, its formation could only be effected by a successive division and multiplication of the latter. In admitting, however, the possibility of the formation of the tubercle in this manner, we must likewise admit that a cell, the nucleus of which is undergoing a process of involution by the presence of bacteria in its protoplasm, is still capable of reproducing its kind. In this case the bacteria would be transmitted from cell to cell by the division of the mother-cells. But, even if this theory regarding the origin of the first tubercle were true, it would still fail to explain the simultaneous arising of hundreds, nay, in many cases, thousands of other tubercles, separated one from another by healthy parenchyma in the same lungs.

In the preceding section of this treatise I have shown that the bacteria tuberculosis first appear in the nuclei of the tubercle cells. Simultaneously, or even previously to the appearance of the bacteria in the nuclei, not only a gradual shriveling in the protoplasm of these bodies, but, in many instances, also a breaking up into several fragments is observed, the whole phenomenon indicating the going on of a process of involution. The same phenomenon is also observed, as I have already stated, in the nuclei of the cells of cancerous, or other neoplastic growth, while they are undergoing a certain kind of

degeneration; and, furthermore, in the nuclei of pus-cells and other leucocytes, as, for instance, the exudation cells of croupous pneumonia, or even in the nuclei of the epithelial cells of the bronchioles, met with in phthisical and pneumonic expectorations. In the nuclei of the tubercle cells this process of involution, manifesting itself by the shriveling and breaking up of the protoplasm of these bodies, is accompanied, as I have shown, by the appearance and development of bacteria tuberculosis.

Now the question may be asked: How is it possible for the tubercle to develop, if a tubercle-cell is rendered incapable of multiplying itself, as soon as bacteria appear within its nucleus? This apparent contradiction is easily explained in considering that the bacteria tuberculosis, even in incipient tubercles, do not appear in the nuclei of the younger tubercle-cells, but rather in those of the cells first formed. Besides, it is not in the nuclei of all the cells of a tubercle that bacteria will be met with; on the contrary, in every tubercle a considerable number of cells is observed, in which the nuclei present their normal form, and where no bacteria can be demonstrated, even after the most careful staining. I have already stated that the nearer the cells of a tubercle are to its cheesy centre, representing the part of the tubercle first formed, the more highly developed are the bacteria, and *vice versa*; though I must say that, now and then, I have observed bacteria in some of the nuclei of the lining cells of alveoli, adjacent to a tubercle, and apparently free from tuberculous growth. These cases, however, are exceptions to the rule. From these observations we may justly presume, that not all cells of the tubercle are in one and the same condition, but that, on the contrary, one part of them may be normal, while the rest is undergoing a process of involution or degeneration; it is in the nuclei of the latter that

the bacteria tuberculosis find the proper soil for their development.

In the preceding pages I have discussed some of the ways and manners in which the bacteria tuberculosis might gain access to the nuclei of the tubercle-cells, while showing at the same time the objections which render these suppositions implausible. But having thus far failed to offer an explanation more satisfactory than those already discussed, the reader would now be justified to ask for the *true* manner in which these bacteria originally get into the interior of these nuclei, where, according to my observations, they are developed. Often, during the course of my investigations, I have put this question to myself, but I must confess that hitherto I have completely failed to answer it satisfactorily, because every attempt to solve this problem leads the inquirer unconsciously to one or the other field of speculation. But, being more accustomed to deal with facts than with theories, or hypotheses, I shall further remain content with my ignorance on this part of the subject, until the facts, which I have observed, will be corroborated and properly explained by other investigators. This, however, will not prevent me from simply pointing out the manner in which alone the origin and development of bacteria in the nuclei of neoplastic cells could be *hypothetically* explained. This is as follows:

The observation of the bacteria appearing in the nuclei, before they are seen in the protoplasm of the cells, without being able to explain, on account of their incapacity of moving spontaneously, the exact manner in which they gained access to these bodies, naturally leads to the suggestion that they must have originated there. The observed fact, however, that these organisms are not present in the nuclei of all the cells of a tubercle, or of any other neoplasm, suggests furthermore, that

the condition of the protoplasm of the nuclei containing bacteria, must be different from that of those nuclei containing none; that this is really the case is proved by the difference observed in the morphological appearance of these nuclei, as I have sufficiently demonstrated before. Now, there are two hypotheses which might explain the origin of the bacteria in the substance of the nuclei. The one consists in supposing that certain germs, too minute to be detected by the microscope, are constantly present in the nuclei of the cells of the organism, to which they were carried by the nutritive fluid of the latter, the liquor sanguinis, and incorporated with the normal substances of nutrition contained in the fluid. These germs, then, remaining dormant during the normal condition of the cells and their nuclei, might be developed into bacteria as soon as degeneration of the latter takes place.

The second hypothesis is identical with H. CHARLTON BASTIAN'S theory of *heterogenesis*, according to which low organisms, such as bacteria, may be directly formed from the protoplasm of animal cells, the vital activity of which is on the wane, or, in other words, undergoing involution, as, for instance, is witnessed in the tubercle of tuberculosis. When Bastian advanced this theory, a number of years ago, it met, like his theory of "spontaneous generation," with a great opposition, because it did not correspond with the predominating idea, that such organisms must be the offspring of a parent of their own kind. For my own part I cannot blindly discard the theory of heterogenesis, for the reason, that it does not appear to me entirely impossible, that the normal arrangement of the molecules of the protoplasm of the nucleus of an animal cell might under certain conditions of the organism be altered in such a manner as to correspond with that of the molecules of vegetable protoplasm; nor is it proved that such a molecular

change in the protoplasm of an animal cell is altogether impossible. On the contrary, it will appear more probable in considering that a difference already exists in the molecular composition of the protoplasm of the nucleus and that of the body of the cell, manifesting itself by the difference in the reactions of certain agents on these protoplasms, as, for instance, acetic acid dissolving the protoplasm of the body of the cell, while it leaves the nucleus unaffected. Furthermore, in staining animal tissues containing bacteria, as sections of tubercle, with aniline colors by Ehrlich's method, this dissimilarity in the molecular composition of the protoplasm of the nuclei and that of the bodies of the cells is rendered very striking by the fact that, while the latter offers no resistance whatever to the decolorizing action of the acids, but readily parts with the color previously absorbed, the former resists to a certain degree, retaining mostly some of the color, though not as much as the bacteria. The resistance of these nuclei to the decolorizing action of the acids even increases, whenever their normal condition is altered. The phenomena just described certainly indicate a similarity of molecular composition existing in the protoplasm of the nuclei of animal cells and in that of the lowest vegetable organisms, such as the bacteria. And in considering this fact a little more closely, the idea,—that under certain conditions of the cell and its nucleus a certain process of involution is set up, by which the constituent particles of the nucleus might, according to Bastian, "individualize themselves" and grow into lower organisms,—will appear less absurd than it has hitherto been regarded. Besides, if it is possible, as we must presume, that at one time or another in the early history of our globe, higher organisms developed from lower ones, while the latter originally arose from a primitive formless protoplasm, then it may not be impossible that

the lowest organisms in nature, represented by the bacteria, might spontaneously arise from the still living protoplasm of the nucleus of an animal cell, which under certain unknown conditions had assumed the nature of vegetable protoplasm. Such a process, if even not probable, is at least possible. In the consideration of a question, like the one before us, however, we should always remember that, though modern science has deeply penetrated into the secrets of nature, many natural processes are still left hidden to us. To these belong the mutual relations of the molecules of animal and vegetable tissues, of which our knowledge is rather deficient, for the reason, that molecules cannot be seen, though it is essential that, in order to render the explanation of those phenomena generally manifested by matter more easy, they should hypothetically exist in our mind. The same is the case with Bastian's theory of "heterogenesis" which, at least in some instances, would satisfactorily explain, as I shall demonstrate directly, the action of certain specific and contagious poisons. To demonstrate this, however, a few remarks on the most important property of the bacterium tuberculosis, namely, that of giving rise to the tubercles of tuberculosis, will be necessary.

Although the question whether the bacterium tuberculosis is in reality the direct inciting cause of tuberculosis in man, is not definitely settled, Koch's discovery of tubercles being artificially produced in the lungs, or other organs of certain animals, by the introduction of this organism into the system of the latter, must at present be regarded as an established fact. The conclusion drawn from this fact is, of course, that the property of producing a specific disease like tuberculosis in the organs of these animals is inherent in the bacterium tuberculosis; though the process by which the pres-

ence of this organism gives rise to the formation of tubercles in the respective tissues, is still obscure. If the production of tubercles in these animals were confined to the inoculation of those bacteria contained in tuberculous matter, the phenomenon might be easily explained by presuming this matter, and not the bacteria, to represent the specific poison. But this is not alone the case, for tubercles will still be produced in these animals by inoculating them with very remote generations of those bacteria, obtained by successive artificial cultures of those originally contained in tuberculous matter. Now, as it thus far appears to be proved, that no other species of bacteria, nor any other substance is capable of producing tubercles in an animal, when introduced into its organism by inoculation, the only satisfactory explanation of the phenomenon appears to be by presuming that the bacterium tuberculosis must have acquired this special property of producing tubercles from the tuberculous matter itself, from which it drew its nourishment during its development; that is to say, that during the development of the organism, the constituent molecules of its protoplasm arranged themselves in a similar manner as those of the tuberculous matter, on which it lived, and that it thus acquired the properties of the latter, that is of producing tubercles; and furthermore, that these properties are during the artificial cultivation of the bacteria, transmitted from generation to generation.

According to Koch the tubercle bacteria are entirely dependent upon the human and animal organisms for their existence; or in other words, they are incapable of living or reproducing out side of the latter. With reference to this assertion we may ask: where then did they originally come

from, if not from the outside world? The most plausible answer for this question may be found in the theory of "heterogenesis," according to which these organisms would originate in the elements of the specifically diseased organ itself. But the bacteria being derived from the latter, it is but natural to presume that they were also endowed with the specific properties of the diseased matter of this organ, manifesting themselves in producing "*alike*," that is, in calling forth the same pathological conditions as were present in the tissues of the respective organ, from which they were derived. In the same way all other specific contagious diseases may be explained by the theory of "heterogenesis."

Although, as may be gathered from the above, I do not deny the contagiousness of tuberculosis in cases where the specific poison is directly transferred from one organism into another, as is done in all the inoculation experiments performed on animals, it nevertheless appears to me very improbable that the numerous cases of tuberculosis in man should have originated in this manner. On the contrary, it is very doubtful that such numbers of tubercle-bacteria, as would be required to produce all these cases, are contained in the atmosphere. Koch, of course, asserts that these bacteria are derived from the expectorations of tuberculous patients, which are left on the floors of rooms or corridors, or on the ground of the streets, and which after getting dry and pulverized by the shoes of people walking over them, are disseminated throughout the air. In examining this proposition a little more closely, it will be found that, notwithstanding the prevalence of tuberculosis, a very small fraction of all the tuberculous expectoration will have a

chance to reach the atmosphere in a pulverized state. In commencing our examination among the better situated classes of society, we will find that the tuberculous patients, for the sake of cleanliness, expectorate into spitboxes instead of upon the floor, and that with the cleaning of these articles of household furniture the expectorations will eventually arrive in the sewers of the street, or in some other channel of drainage. If the patient is confined to bed, he will generally be provided with some sort of a vessel, into which he may expectorate; and if he should even make use of a handkerchief, the expectoration will hardly be allowed to dry and to be pulverized, but rather be submitted to the action of the soap-water in the washtub. The same fate the tuberculous expectorations will meet with in hospitals, though in large public institutions of this kind it may happen, now and then, that a tuberculous patient soils the floors of a corridor with his expectoration, which, however, will hardly be left to dry and to be pulverized. Hence it would be only in the lowest strata of society, where cleanliness is unknown, that tuberculous sputa might reach the atmosphere in the form of powder. In all other cases the bacteria tuberculosis, instead of being disseminated throughout the atmosphere, will ultimately arrive in the gutters and sewers of the street, to be carried to the river or ocean, where they will be incapable of living or thriving, because their existence, according to Koch, solely depends upon the human or animal organism. The same will happen to the tuberculous expectorations left on the pavements of the streets, where they will be washed away by the rains of the season. We may, therefore, presume that, as I have remarked before, but a very small fraction of all the bacteria contained in the ex-

pectorations of tuberculous patients, will ever come to float in the atmosphere. But even this small fraction, which under favorable circumstances may come to be suspended in the atmosphere, will have but very narrow chances to reach the interior of the human lungs, for the reason that, being, as just mentioned, incapable of living or of increasing their numbers by reproduction outside of the human or animal organism, they will soon be dispersed and swept away to unknown regions by the winds, or removed from the atmosphere by every rain, and like their brethren, finally arrive in the various channels of drainage.

I might still further enlarge upon this subject, but I trust that the foregoing remarks will suffice to show that the numerous cases of tuberculosis cannot be regarded as having been caused by bacteria tuberculosis floating in the atmosphere. In those rare cases in which the disease has in reality been communicated from one individual to another, it was undoubtedly accomplished by the medium of the expectoration itself, which in the form of spray—as may happen during the act of coughing or speaking or even kissing—issues from the mouth of the tuberculous individual to be inhaled by the lungs of another person. Besides the immunity from tuberculosis, generally exhibited by the resident physicians and nurses of hospitals, is another proof to show that this disease is but rarely communicated by the sole medium of the atmosphere; as an instance of this fact I simply point, as has been done by other authors before me, to the well known statistics of the “Hospital for Consumption,” at Brompton, in England.

Notwithstanding the popularity which Koch’s theory of the bacterium tuberculosis being the direct and sole cause of

tuberculosis, soon acquired after the discovery of this organism, and to a certain extent still holds, it cannot be denied that since that time, so far as may be gathered from the current medical literature, the number of those physicians who doubt the correctness of this theory, has been steadily increasing. Thus some of the most distinguished clinicians, being unable to indorse the whole original theory, have altered it in order to render it harmonious with their own practical experience, and with the old-established fact that tuberculosis is hereditary in its nature, depending upon a special diathesis which is transmitted from the parent to the offspring. Accordingly, Koch's original theory, teaching that the bacterium tuberculosis was capable of producing tuberculosis without regard to predisposition in every individual, has been modified in such a manner as to read now: *Bacterium tuberculosis produces tuberculosis when associated with tubercular diathesis, but is harmless in the absence of the latter; and vice versa, tubercular diathesis leads to tuberculosis if associated with bacterium tuberculosis, but is harmless without this organism.*

Presuming that the reader is familiar with the numerous articles, published in various medical journals, and of which the authors either advocate this theory, or even regard the bacterium tuberculosis as a product of the disease itself, I forbear to enlarge furthermore upon this subject; though I may say that the modification of Koch's original theory rather appears to me as a loss of faith in the capacity of this organism of causing the formation of tubercles. But, notwithstanding the doubts which still exist in regard to this capacity of the tubercle-bacterium, its presence in the tissues of the human lungs, or other organs, must be regarded as pathogno-

monic of the disease; though cases of tuberculosis are met with in which none of these bacteria are detected in the sputum of the patient, for the reason, that no cheesy centers, containing liberated bacteria tuberculosis, have as yet been formed.

In the foregoing treatise on the relations of the bacterium tuberculosis to the human lungs, or other organs, I have presented the subject to the reader in the light in which it has appeared to me, and, as much as possible, without any prejudice against one or the other theory concerned therein. As far as I know, I have, though unjustly, been regarded as an opponent to the theory of "living germs of disease," because I failed, as in the case of yellow fever, to detect living organisms, to which any signification concerning the cause of the disease might have been attached, in the tissues of the organs which I examined. On the contrary, I have always believed in the possibility of minute living organisms being capable of producing disease by their presence in the blood and tissues, though, in some instances, including tuberculosis, I have doubted the probability of this being the case, for the reason, that the facts, upon which the theory was based, were insufficient to explain all the phenomena manifested by the respective diseases. In those cases, however, where the observed facts covered, or explained all these phenomena, I have never hesitated to acknowledge the facts presented. But, I am unable to enthusiastically receive any new theory, founded upon one or the other observation, on mere faith, though it might be in accord with the already prevailing ideas of the day. Not being naturally inclined to enthusiasm—a passion nearly allied to fanaticism—I prefer in such cases to coolly reflect upon every detail of the subject, and, if possible, to examine it myself, before forming a definite opinion. Every new theory

should be received with caution, and not be accepted as true, before it has been thoroughly examined in all its aspects. For nearly thirty years, I have myself been engaged in original studies, but I must confess that, knowing how much every investigator is inclined to interpret some of his observations so as to correspond with one or another favorite idea, I frequently hesitate to draw conclusions from any observation, before I have thoroughly examined and considered the subject in all its aspects. During the progress of science, many theories have been formed, and subsequently abandoned upon the discovery of new and more substantial facts, that explained phenomena which the old theory failed to do. It is, therefore, not impossible, that the same may happen to the now prevailing germ-theory, and that thus the whole problem of the "germs of disease" may be solved in some at present unknown way.

EXPLANATION OF THE ILLUSTRATIONS.

Fig. 1.—Bacteria tuberculosis in tuberculous expectoration; *a*, deeply stained; *b*, feebly stained. Magnified 1090 diameters.

Fig. 2.—Bacteria met with in urine, the description of which will be found in the text.—Magnified 1,090 diameters.

Fig. 3.—Represents a small part of the periphery of a miliary tubercle with several of the neighboring non-tuberculous alveoli; *a*, shriveled nuclei containing more or less bacteria tuberculosis; *b*, epithelial cells of the non-tuberculous alveoli; *c*, inter-alveolar septa with the nuclei of their multiplied connective-tissue cells, derived from the adventitia of their vessels, etc.; *d*, small portion of the periphery of the tubercle; *e*, young tubercle, arisen from the wall of a large alveolus, or perhaps infundibular cavity, and, contrary to the

rule generally observed, has undergone the coagulation necrosis.—Magnified 625 diameters.

Fig. 4.—Represents a portion of a miliary tubercle near its cheesy centre; *a*, bacteria tuberculosis, developing in the nuclei of the tubercle cells; the outlines of the cells are not seen, on account of the object being mounted in Canada balsam and very highly illuminated; *b*, the same nuclei out of focus; *c*, inter-alveolar septa; *d*, portion of the cheesy centre of the tubercle.—Magnified about 1,090 diameters.

Fig. 5.—Represents a small portion from a large tubercle from the lungs of a boy, sixteen years old, and affected with miliary tuberculosis of the lungs, liver, spleen, kidneys, and lymphatic glands; *a*, bacteria tuberculosis, developed in the nuclei of the tubercle cells; *b*, the same out of focus; *c*, degenerated interstitial connective tissues, leading the central part of the tubercle; *d*, groups of bacteria, commencing to enlarge in their dimensions; *e*, aggregations of bacteria tuberculosis.—Magnified about 1,090 diameters.

Fig. 6.—Bacteria tuberculosis, contained in the nuclei of the muscular and of the connective-tissue cells in the wall of a bronchioles; *a*, nuclei of the connective-tissue cells; *b*, nuclei of the muscular-tissue cells.—Magnified about 1,090 diameters.

Fig. 7.—Bacteria tuberculosis contained in the nuclei of the cells of indurated tuberculous tissue.—Magnified about 1,090 diameters.

Fig. 8.—Represents a small portion of the parenchyma of the liver, bordering directly the periphery of a miliary tubercle; *a*, nuclei of hepatic cells, still presenting a normal appearance; *b*, nuclei with bacteria developing in their interior.—Magnified 625 diameters.

Fig. 9.—Represents a portion of the centre of a miliary

tubercle of the liver, having undergone coagulation-necrosis; *a*, fissures, very likely representing the lumen of contracted capillaries; *b*, groups of bacteria tuberculosis with the remains of the protoplasm of the nuclei, in which they were developed; *c*, aggregations of these bacteria.—Magnified 625 diameters.

Fig. 10.—Represents some of the anatomical elements of a miliary tubercle of the liver; *a*, group of normal hepatic cells from near the border of the tubercle; *b*, a pair of hepatic cells from the periphery of the tubercle, the nuclei of which have commenced to shrivel; *c*, a number of nuclei from the cheesy centre of the tubercle, the margins of which are shriveled in a higher degree and broken up into fragments; *d*, ultimate minute fragments of nuclei, some of which have assumed the form of granular filaments.—Magnified about 1,090 diameters.

Fig. 11.—Represents a small portion of a leprous liver; *a*, normal nuclei of hepatic cells; *b*, degenerating nuclei with shriveled margins; *c*, bacteria lepræ, developing in the nuclei of hepatic cells; *d*, bacteria lepræ, liberated by the degeneration of the protoplasm of the nucleus, and disseminating throughout the likewise degenerated protoplasm of the body of the cell.—Magnified 625 diameters.

Fig. 12.—Bacteria lepræ, as met with in the blood of some lepers.—Magnified about 1,090 diameters.

Fig. 13.—Represents a minute portion of the pars reticularis of the corium of the skin from the margin of a leprous tubercle; *a*, nuclei of connective-tissue cells containing bacteria lepræ in their early stage of development; *b*, neoplastic connective-tissue cells in an advanced stage of fatty degeneration, containing fully developed bacteria lepræ between the fat-globules.—Magnified 625 diameters.

Fig. 14.—Represents a minute portion from the middle of the same leprous tubercle, in which the bacteria lepræ are seen in their different stages of development; *a*, bacteria in the nuclei of the unchanged connective-tissue cells; *b*, bacteria slightly more developed; *c*, the nucleus has disappeared by degeneration and the bacteria lepræ are seen fully developed in the protoplasm of the cell, exhibiting their granular nature; *d*, fully developed bacteria lepræ between the fat-globules of the degenerated cell.—Magnified 625 diameters.

Fig. 15.—A number of degenerated leprous cells higher magnified, showing to a better advantage the morphological character of the bacteria lepræ.—Magnified about 1,090 diameters.

HYDROPHOBIA AND IMAGINATION. By W. H. FORWOOD, M. D.,
Major a Surgeon, U. S. Army.

On a beautiful moonlight evening, September 10th, 1868, the military post of Fort Larned, Kansas, was suddenly invaded by a mad wolf. The post was situated in those days in the midst of a vast rolling prairie far from any human habitation, save the encampments of roving Indian tribes, and on the plains in all directions were great bands of buffalo, wolves and other wild animals.

The night was clear and warm, and about 10 o'clock the patients in the ward of the little one-story adobe hospital were startled by the entrance of a large gray wolf. Corporal McGillicuddy, 3d Infantry, in hospital for sore eyes, rose up in bed, thinking to scare the animal off, but he was instantly attacked and severely bitten in the left hand and right arm, when the wolf rushed out through the open door and disappeared as suddenly as he came.

A moment later he was at a tent near by where two women were sleeping, and tore a large hole in the side of the canvas, leaving the marks of his teeth on the bed rail, without doing further damage.

His next appearance was at my own quarters, where he ascended the porch, bounded through the open window, tearing a large piece out of the curtain, sprang over the lounge on which I was lying at the time, and encountered my large Newfoundland dog "Prince," in the middle of the room. The conflict that ensued was short and sharp; a furious rush, a few vicious snaps of his powerful jaws, and the wolf was off through the hall and out at the back door, apparently satisfied with the mischief he had done without any desire to fight the battle to a finish. His course from this point was a bee line across an open space of about 100 yards to the house of the Indian agent, where a number of persons were assembled, enjoying the beautiful evening on the veranda.

Lieutenant Thompson, 3d Infantry, was seated on the steps and saw the wolf approach. He came at a run, and without hesitating seized the officer, first by one leg and then the other, inflicting quickly several severe bites, and was gone in a moment at the same rapid pace in the direction of the cavalry stables.

On a cot in the open air in front of the stables, Private Mason, of the 10th Cavalry, was lying with his shoes and stockings off, awaiting his turn to go on post as one of the guard. The wolf seized him by the bare foot and drove his enormous canine tooth entirely through between the metatarsal bones from the instep to the sole.

The sentinel walking post across the way had not sufficient time to bring his gun to bear before the infuriated animal was upon him, and fired over its back as it plunged headlong be-

tween his legs, throwing him to the ground, and disappeared toward the hay stacks, leaving no more serious damage than an ugly rent in the leg of the soldier's pantaloons.

The guard at the hay stacks saw the wolf coming in time and making a lucky shot sent a bullet through his chest into the spinal column, killing him instantly. He was a large male specimen of his kind, very numerous at that time on the surrounding prairies. His mouth was covered with a frothy saliva, tinged with blood, traces of which he had left on every object touched in his entire round of the post. Indians relate similar instances of mad wolves entering their camps, and say they always remain and continue their depredations as this one did until killed.

The wounds of the three men bitten were promptly examined and all thoroughly washed in warm water and cauterized with the nitrate of silver. Lieutenant Thompson had numerous scars on both legs from the ankles to the knees, inflicted *through the ordinary clothing*. Private Mason's foot bled freely and was soaked for some time in several changes of water and then cauterized as deeply as possible from both openings of the wounds. Corporal McGillicuddy had the flesh stripped from his left little finger, leaving the bone almost bare. In the right arm the bites were deep, and except near the wrist were made through the covering of his cotton night shirt. I urged the immediate amputation of his finger at the metacarpophalangeal articulation, but he positively refused to give his consent. I suggested hydrophobia, but he laughed at the idea, saying wolves never went mad, and that there was no danger. It was not until the second or third day after the accident that he would consent to the amputation, when ether was given and the operation done. No attempt was made to

treat the wounds of the dog, as his heavy coat of shaggy hair made it difficult to find them.

All the wounds, including the amputation, healed quickly and kindly. Lieutenant Thompson and Private Mason remained free from any unpleasant symptoms and are still living, although left to their own reflections, as they were for many months in that dreary and monotonous place, far from home and friends and fully cognizant of the fate that befell the dog and the other man, and told many stories by the Indians of the fatal effects of such accidents, they could not have been entirely free from anxiety.

Poor old Prince remained well and happy as usual until about the tenth day, when for the first time in years he showed signs of serious illness. He was dull, mopy, did not care to go out and refused all food or drink. When water was placed before him he appeared to want it, but would make no effort to swallow. He was securely chained in a comfortable shady place on the grass and carefully attended, but did not improve. On the afternoon of the second day he seemed very nervous, restless and unnatural in disposition and still refused to eat or drink. At this stage of the case an officer whose anxiety for the safety of his children was too great to tolerate any further experiments with a mad dog in the vicinity of his quarters, put an end to all further observation by shooting him through the head; but there was no doubt left in my mind as to the nature of the disease.

McGillicuddy was an old soldier, and I managed to keep him pottering about the hospital on the pretext of a slight granulation of the lids. He was otherwise perfectly well and cheerful, and showed no trace of fear or anxiety concerning his wounds. He knew that Thompson and Mason were well and on duty, and the fate of Prince was carefully kept from

him. He had been on the frontier for many years and scouted the idea of a mad wolf as something too absurd to mention.

On the morning of the thirty-first day after the accident, in making my rounds of the ward I found the corporal lying on his bed. He did not get up and stand "at attention" as was his pride and custom, answered my questions in monosyllables, and was evidently out of sorts. At nine o'clock that evening the steward sent word that McGillicuddy was acting strangely and seemed quite ill. He was nervous, restless, cross, and wore an anxious expression. He complained of a smothering or choking sensation about the chest and throat, which he attributed to the *ether* administered when his finger was amputated, and scolded me roundly for having made him take it. I noticed every now and then a peculiar spasmodic action of the diaphragm—a succession of two or three quick, short, jerking movements—giving rise to a sort of sob or sigh, such as may be noticed in young children at the end of a long fit of crying. This occurred once in two or three minutes when quiet or oftener when disturbed and was quite characteristic. Some little simple sedative mixture was prepared and offered him in a glass, but he discovered to his surprise that he could not take it, nor even think of doing so without great distress. After a little persuasion he took the glass, holding it as far from him as possible, and in great agitation he assured me he could not swallow. Finally, after several attempts, he succeeded in bringing the glass with a trembling hand almost to his lips, then throwing it violently from him fell back in convulsive excitement, sighing rapidly and deeply as above described. From this time the man never swallowed a particle of anything until just before the close, when the attendants thought they had succeeded in getting a few drops of water down his throat from a teaspoon. This abstinence was not

from any obstinacy on the part of the patient, but because swallowing was impossible and the attempt distressed and excited him more than almost anything else.

The spasmodic disturbance beginning in the muscles of the diaphragm and throat extended in the next twenty-four hours to the chest, back and extremities, and the nervous sensitiveness increased until it became extreme. The least disturbance, the slightest noise, the gentlest touch sufficed to bring on spasmodic excitement. So far as the muscular system was concerned these convulsive attacks bore a striking resemblance to those caused by poisonous doses of strychnine. Every fibre of the muscles seemed powerfully contracted for a time, then slowly relaxed, and the patient, whose mind remained clear throughout, would beg those about him to keep still and make no noise, that the intervals of comparative rest might be a little longer. The effort to talk distressed him, and he therefore spoke as little as possible, sometimes complaining of thirst and a burning sensation in the throat and mouth and of the frightful pains that accompanied his attacks. Death closed the scene at the end of seventy hours after the first noticeable symptoms appeared. Not the slightest sign of fear was exhibited at any time, and almost the last word the patient uttered was a complaint against the doctor for giving him ether, which he believed to be the main cause of his illness.

Much has been written of late by prominent men in the medical profession tending to show that true rabies in man is a myth, the so-called cases of hydrophobia being merely the result of fear and imagination. It seems to me that a sufficient refutation of this theory is the fact that rabies is communicated, through the bite, from one animal of the canine species to another, and from these to beasts of other species, as horses, cows, pigs, sheep, etc. It will hardly be claimed that fear or

imagination plays any important part in determining the result in such cases as these, which are numerous and well authenticated; and if thus communicable to the lower animals, why not to man also? It is certain that the disease originating in dogs and wolves and characterized by the definite and well recognized symptoms long known under the name of hydrophobia is no myth but a painful and invariably fatal reality. Precisely the same symptoms are transmissible from one dog to another through the bite, and similar symptoms to other animals, each according to its species. Those produced in man are characteristic and distinguishable from all other known diseases. In order to a clear understanding of the matter it is necessary to bear in mind the following important points:

1st. Rabies in the canine species, though probably liable to epidemic outbreaks, is quite rare, but many other diseases of dogs have been mistaken for it, thus giving rise to numerous spurious cases and much unnecessary alarm.

2nd. The bite of a rapid dog does not always, nor even in a majority of cases, infect the animal bitten, just as vaccination does not always "take" in children, although the virus may be good. It may not be introduced into the blood or may be washed off before it has been absorbed, or possibly the system may be from some unknown cause protected against its effects. Of a given number of animals bitten all remain well for a longer or shorter period, and then a small proportion sicken and die and the rest show no ill effects other than those resulting from the slight wounds inflicted, which are no more serious than if made by a healthy dog. But, once the symptoms of hydrophobia appear, death is inevitable. There is no middle ground. Recovery, so far as any well authenticated instance to the contrary is concerned, is *prima facie* evidence of non-infection and erroneous diagnosis. PREVENTION by means of

washing, cauterization, etc., of the wounds is the only effectual treatment.

Imagination plays an important rôle in medical pathology and fear is a serious complication in many diseases, and it is to be expected that among intelligent beings, fully conscious of all the horrible consequences liable to follow the bite of a rabid animal, there should be more or less nervous disturbance and mental agony which in certain persons may lead to serious or even fatal results; but this will not hold good in the case of babies, idiots, insane persons or any of the lower animals, quite as liable to hydrophobia as the most imaginative men or women in the full possession of their mental faculties.

505 La Salle Avenue.

INTUBATION OF THE LARYNX AS A SUBSTITUTE FOR TRACHEOTOMY IN THE TREATMENT OF PSEUDO-MEMBRANOUS LARYNGITIS; WITH A REPORT OF EIGHTY-THREE CASES. *By F. E. WAXHAM, M. D., Professor of Diseases of Children, College of Physicians and Surgeons, Chicago.*

[Read before the Chicago Medical Society.]

A little over one year ago it was my privilege to present to this SOCIETY Dr. O'Dwyer's instruments for intubation, and to illustrate the operation upon the cadaver. The instruments were then imperfect, and a few trials and accidents proved the necessity for various modifications. First, the tubes were plain, with no shoulders, and they were too frequently rejected while coughing or vomiting. There was nothing to keep them in position but their length and weight.

Again, the heads of the tubes were too small, and after losing one in the trachea we were thoroughly convinced of the necessity for a change. The first important modification con-

sisted in the addition of a shoulder to make them self-retaining. This shoulder consisted of a slight swelling or enlargement at the centre of the tube. Still they were not self-retaining, being frequently rejected by strong and vigorous children. The diameter was still further increased, and although they will now occasionally be rejected, yet they are as large as is consistent with safety. Another modification consisted in the enlargement of the head of the tube to prevent its falling into the trachæa. A third, and, I believe, a very important modification,* has been the construction of the tubes with thinner walls, so that they are lighter and the calibre larger, giving greater breathing space and a better opportunity for the expulsion of false membrane. These tubes, which I now have the honor of presenting to you, will illustrate the various changes that have been made. In addition to these tubes for children I wish to present an adult tube which Dr. O'Dwyer has very kindly sent me. You will notice the great size and weight of this tube.†

The first extractor, and the one presented to you a year ago, consisted of an instrument with two jaws, opened and closed by a spring, one of the jaws entering the tube and the other passing outside. By pushing on the spring the tube is grasped between them.

Dr. O'Dwyer has had an extractor constructed on a somewhat different principle. The jaws while closed are passed into the tube and by pressing on the spring they are separated, thus holding the tube while it is extracted. This extractor has been very kindly furnished me by Hugo Keller, of New York.

* Made by Sharp and Smith, of Chicago.

† Which would seem objectionable, but which Dr. O'Dwyer claims to be an advantage, as it prevents expulsion.

No change has been made in the introducing instrument or the method of placing the tube.

A few words, however, in regard to the manner of introducing the tube may not be out of place. The operation has been so frequently described that it is unnecessary to go into details. I wish simply to refer to the relation of the index finger and the end of the tube while it is being introduced; a practical point which will be of great service to the inexperienced. There are two methods of placing the tube: we may introduce the index finger of the left hand until we feel the aperture of the larynx and then guide the tube into it, or we may simply reach the epiglottis, passing the end of the tube gently over it and then making an abrupt turn, when the tube, if in the median line, will slip into the larynx. The practical point is this: when the index finger has been introduced, the end of the tube, in either case, should be passed under the finger, not over it or by the side of it, but directly under it. In extracting the tube, the finger should be introduced over the epiglottis until it reaches the top of the tube; we then feel for the opening of the tube, and the extractor is introduced under the finger in exactly the same manner.

I would here *insist* upon the importance of practice upon the cadaver before attempting the operation upon the living child. While the expert will occupy scarcely five seconds in placing the tube, and with very little irritation and no injury, yet the inexperienced will often completely fail, causing the little patient great distress, the friends agony, and the physician great embarrassment. Indeed repeated and unsuccessful attempts upon the frightened, struggling, suffocating child are nothing short of cruelty. On two occasions it has been my misfortune to witness unfavorable terminations of apparently hopeful cases from detachment of large masses of membrane

below the tube. As our failures are oftentimes more instructive to us than our successes a history of one of these cases may not be uninteresting.

On March 8th I was called to see a boy, eight years old, in the last agonies of suffocation from diphtheritic laryngitis. Intubation was quickly performed and with prompt and entire relief to the distressing symptoms.

	4:30 P. M.,	pulse, 130—	temp., 101—	respiration, 24.
	9:00 “ “	112—	“ 102—	“ 24.
March 9th,	9:00 A. M.,	“ 120—	“ 102—	“ 20.
	5:00 P. M.,	“ 112—	“ 103—	“ 24.
	9:00 “ “	110—	“ 101—	“ 24.
	“ 10th, 9:00 A. M.,	“ 112—	“ 101—	“ 24.

The patient seemed bright and perfectly comfortable, and took an abundance of nourishment without difficulty. There was every indication of a favorable termination. At about 4 o'clock, March 10th, a little over two days after the operation was performed, he had a “choking spell,” but before my arrival he had succeeded in expelling a piece of membrane and was perfectly easy. Careful auscultation, however, revealed a bronchial complication. A mustard poultice was applied to the chest, followed by flaxseed poultices, and an expectorant mixture given. Fearing another attack of suffocation from detachment of membrane below the tube, I hastened home to make arrangements for remaining with the child all night. Hardly had I reached home before I was again summoned in the greatest of haste, the messenger stating that the boy was “choking to death.” Upon arriving a few moments later I found the message too true, and upon entering the room the father exclaimed, “Oh, Doctor! you are too late, he is dead.” Respiration had ceased and he was blue and pulseless. Never-

theless artificial respiration was resorted to and in a few moments he was resuscitated, although the respiration was labored and imperfect. The tube was at once extracted; during the operation the child passing into a convulsion. Chloroform was given to control the convulsion and help was summoned. In response to my urgent request for aid Drs. McWilliams and Sherry kindly came to my assistance. The child was placed upon the table, the trachea opened and a mass of membrane several inches in length removed. The patient revived, took nourishment well and seemed comfortable. There was, however, a rapid extension of the membranous exudate and the child died within twenty-four hours from bronchial obstruction. Another case terminated fatally in much the same manner, from peeling up of membrane directly after the removal of the tube.

This indeed is a danger that may be justly dreaded. In order to overcome it I have devised a tracheal forceps with which we may enter the trachea, by the way of the mouth, and remove the mass of membrane causing the obstruction.

There is one other source of danger. Occasionally a child will utterly refuse to take nourishment, and rectal enemas fail to support the patient sufficiently. To obviate this danger, I have had constructed a feeding-flask, consisting of a nursing bottle with a tube extending from it to the bulb of a Davidson syringe; this in turn is connected with an œsophageal tube. The tube is introduced into the œsophagus, and by pressing upon the bulb the contents of the bottle may be quickly transferred to the stomach. †

In considering whether we have a substitute for tracheot-

* Made by Sharp & Smith. † Made by E. H. Sargent & Co.

omy in tubage of the larynx, the first and most important question that will arise will be in regard to the comparative success of the two methods. Our text-books generally give one out of three as the average proportion of recoveries from tracheotomy, but that this percentage is too high, few I am sure will deny. It must be remembered that these results are obtained from reports of a few of our most successful tracheotomists. The thousand physicians who have met with inferior success never publish their cases and are never heard from excepting when they raise their voices against early operations. I have, myself, met with discouraging results with tracheotomy, notwithstanding that every patient had the most watchful care and attention, I know of one physician, not of this city, however, who has performed tracheotomy fifty times with but two recoveries, and another, noted for his skill as a surgeon, who has operated twenty times without saving a single patient, and another who has operated fourteen times without success, and another eight times without a recovery, and still another fifteen times with but one recovery, making one hundred and seven cases with but three recoveries.

know of one of our eminent surgeons who delegates the operation to others when called upon to perform it on account of his loss of confidence in the procedure. Knowing these facts too well, I have often wondered why our text-books should have placed so high a value upon tracheotomy, and indeed have sometimes questioned whether the operation is justifiable.

In order to obtain the true status of tracheotomy in Chicago I have by personal inquiry, and by postal card, col-

lected the following statistics, and have been surprised at the result. These inquiries disclose the fact that while we have many physicians who rarely meet with success and who have no faith in the operation, yet there are others who have done remarkable work.

These statistics show that one physician has performed the operation

Another	3 times with 3 recoveries.	7 times with 3 recoveries.	Another	3 times with 3 recoveries.	2 times with 0 recoveries.
"	33	" " 9	"	4	" " 2
"	25	" " 5	"	1	" " 0
"	30	" " 6	"	3	" " 1
"	12	" " 5	"	2	" " 1
"	20	" " 0	"	5	" " 1
"	14	" " 0	"	3	" " 2
"	8	" " 0	"	3	" " 2
"	15	" " 1	"	1	" " 0
"	16	" " 1	"	3	" " 1
"	15	" " 3	"	1	" " 0
"	9	" " 1	"	2	" " 0
"	1	" " 0	"	4	" " 1
"	1	" " 0	"	2	" " 0
"	4	" " 0	"	2	" " 0
"	1	" " 0	"	2	" " 1
"	3	" " 0	"	6	" " 1
"	8	" " 2	"	1	" " 1
"	1	" " 1	"	3	" " 0
"	2	" " 0	"	1	" " 0
"	5	" " 1	"	1	" " 0
"	3	" " 1	"	3	" " 0
"	4	" " 1	"	2	" " 0
"	1	" " 0	"	2	" " 0
"	1	" " 0	"	2	" " 0
"	3	" " 0	"	2	" " 0
"	2	" " 1	"	2	" " 0
				306	58

Making a total of 306 cases with 58 recoveries, or a percentage of 18.95.

In one hundred and thirty-eight cases the ages of the patients were as follows:

I was	2 yrs. and 2 mo.	9 were	2 yrs.	
I "	7 mo.	2 "	2 "	and 4 mo.
I "	13 "	4 "	2½ "	
I "	12 "	19 "	3 "	
I "	20 "	7 "	3½ "	
I "	2 yrs. and 5 mo.	14 "	4 "	
I "	2 " " 3 "	16 "	5 "	
I "	3 " " 3 "	3 "	5½ "	
I "	4½ "	16 "	6 "	
I "	5 " and 7 mo.	6 "	7 "	
I "	6½ "	8 "	8 "	
I "	7½ "	5 "	9 "	
I "	19 "	4 "	10 "	
I "	24 "	2 "	12 "	
6 were	18 mo.	3 "	13 "	

This gives an average of 5 years and 1 month. Where the cause of death has been referred to, extension of the membrane into the bronchial tubes has been given most frequently.

In contrast to these statistics it is my pleasure to report 83 cases of intubation with 23 recoveries, or 27.71 per centum. Three of these cases were reported to me by Dr. C. P. Caldwell, five by Dr. Ingals, seven by Dr. Strong, ten by Dr. Richardson, and fifty-eight were under my own care. The ages were as follows:

No. of cases.	Age.	Recoveries.	No. of cases.	Age.	Recoveries.
1	9 mo.	0	12	3 yrs.	2
1	11 "	0	1	3 " and 4 mo.	1
1	13 "	0	1	3 " " 6 "	0
3	14 "	1	11	4 "	5
1	15 "	0	1	4 " " 9 "	1
1	16 "	0	3	4½ "	0
1	17 "	0	9	5 "	4
1	18 "	1	6	7 "	1
1	20 "	1	1	7½ "	0
7	2 yrs.	2	2	8 "	1
2	2 " and 1 mo.	1	1	11 "	0
2	2 " " 2 "	1	7	—	0
2	2 " " 3 "	0			
3	2 " " 6 "	1			
1	2 " " 9 "	0			
			83		23

Average age, 3 years and 7 months.

It will be observed that 11 cases with 3 recoveries were under 2 years of age, 28 with 8 recoveries were under 3 years, 14 cases with 3 recoveries between 3 and 4 years, 15 cases with 6 recoveries between 4 and 5, 9 cases between 5 and 6 with 4 recoveries and 10 cases with 2 recoveries between 7 and 11 years.

It will also be observed that while the percentage of recoveries from tracheotomy in the 306 cases reported has been 18.95, that from intubation has been 27.71. This, too, represents, not simply recoveries from the operation but entire recoveries from the disease which rendered the operation necessary.

Of the 58 cases coming under my care, 20 were actually moribund when the operation was performed, many of them entirely unconscious, and 40 were bad cases of diphtheria, characterized by severe constitutional symptoms and extensive exudation in the pharynx as well as in the larynx. In 18 cases the exudation in the pharynx was slight, but in every case, without exception, false membrane was expelled, either in the form of muco-pus, shreds or casts. In every case the operation was performed to prevent impending suffocation and the cases pronounced hopeless without surgical interference. In addition to the recoveries cited above, the operation was perfectly successful in 18 others, although the patients died. Thus 4 died perfectly easy, before the removal of the tube, from the severity of the diphtheritic disease; 3 died easily from one to several days after the removal of the tube, from exhaustion incident to the disease; 1 died from paralysis of the heart; 1 from uræmic convulsions; 3 from pneumonia, resulting from hypostatic congestion, and 6 from pneumonia, resulting from unfavorable surroundings. These cases added to those where entire recovery occurred and the result 41, or 49.39 per centum represents the proportion of cases in which the operation was

successful and entirely satisfactory; the remaining cases dying generally from extension of the membrane into the bronchi.

Many of these cases were delegated to me because they were considered too young or too unfavorable for tracheotomy. Many of them were young, nursing infants; and although the operation can rarely succeed, yet the fact that we have been able to save one child but fourteen months old and another of twenty months should be an encouragement always to perform the operation, when indicated, without regard to age. One of these cases was so truly remarkable that a brief history may prove instructive.

May 5th I was called to perform intubation upon a nursing infant of fourteen months, a patient of Dr. Lawless. The infant had just recovered from measles when taken with diphtheria. The pharyngeal deposit was slight, but quickly extended into the larynx. Prof. Steele, who assisted in the operation, expressed the opinion that the infant could live but a few hours without an operation. Intubation was quickly performed, and with instant relief to the urgent symptoms.

Considerable mucus and softened membrane were expelled, and the little patient at once passed into a quiet sleep. Milk was drawn from the breast and given the infant from a spoon for the first two days, after which time it was able to nurse. For two days the little patient did remarkably well, appearing bright and strong and coughing with considerable force.

With the assistance of Drs. Lawless and Clendenning the tube was removed after being in position for two days, but it was necessary to at once reintroduce it. On the third day the strength began to fail, and the patient seemed to be doing poorly. Mucus and softened membrane would collect in the tube and the infant had not sufficient strength to expel it. At

the expiration of the third day the tube was removed, thoroughly cleansed and reintroduced. The infant was in a bad condition, and little hope of recovery was entertained. At about 2 A. M., May 9th, I was called to see the little patient again, and found that it had been having convulsions. The breathing was embarrassed and rapid, and evidently indicated obstruction of the tube. The tube was again removed, after having been in position three and a half days. The respiration, although very rapid, numbering 70 per minute, was comparatively easy, and the tube was not reintroduced. Warm flaxseed poultices were applied to the chest, and the next morning there was marked improvement. Convalescence proceeded slowly but surely, and in a few days the little patient was entirely out of danger and made a perfect recovery.

Many of these cases have been moribund, pulseless, limp, and unconscious when the operation was performed. The history of a single case will illustrate the unfavorable condition of many of these cases.

May 6th I was called by Dr. Clendenning to perform intubation upon a child three years old. Upon arriving we found the child dying. It was blue, cold, pulseless and unconscious. An occasional gasp was the only sign of life. A tube was quickly introduced, not even producing gagging or coughing. Artificial respiration was performed, the child thoroughly rubbed, and after working vigorously for half an hour we were gratified to see the child open its eyes and look about. A few teaspoonfuls of brandy now excited coughing and we soon had the throat free from mucus and softened membrane. The child took nourishment well, the pulse returned, the warmth returned to the surface and the little patient was left sleeping quietly and comfortably. The

next day the child was bright and happy, breathing easily, and no one would have imagined that there was a tube two inches in length in its throat, or that the day before it was at death's door. The patient did remarkably well for three days, giving every promise of recovery. The membrane, however, gradually extended into the bronchi, and the child died four days after the operation was performed. Although the prognosis is far more unfavorable where the patient is moribund, yet that they may be perfectly resuscitated I have proved over and over again, and it has been my pleasure to save one patient in such a desperate condition when the operation was performed. In conclusion I would say, that intubation possesses many advantages over tracheotomy. The ease and rapidity with which the operation can be performed, the perfect comfort of the patient and the rapid convalescence after the removal of the tube are in great contrast to tracheotomy. In addition to this, intubation is more successful. While the record of recoveries after tracheotomy in Chicago has been 18.95 per cent, that of intubation has been 27.71. Again, one of our most successful tracheotomists in Chicago has performed tracheotomy 33 times with 9 recoveries in a period of twenty years. That I have performed intubation more times and saved more lives in a single season is still further evidence of the superiority of the new operation over tracheotomy.

In closing this paper I would take occasion to thank the members of the profession of this city for the many courtesies extended to me. To the following gentlemen I am particularly indebted and would refer to them as to the accuracy of this report: Dr. Steele, Dr. Sullivan, Dr. Hatfield, Dr. Casselberry, Dr. Starkey, Dr. Behrend, Dr. H. T. Byford,

Dr. Richardson, Dr. Dahlberg, Dr. Appleby, Dr. Helm, Dr. Bosworth, Dr. Nelson, Dr. Kosakowski, Dr. Valin, Dr. Chas. E. Caldwell, Dr. Ogden, Dr. Holroyd, Dr. Pierpoint, of Englewood, Dr. Quine, Dr. Miller, of Kensington, Dr. Willard, Dr. C. P. Caldwell, Dr. Dyas, Dr. Berry, Dr. McGinnis, of Brighton, Dr. Hoadley, Dr. Nutt, of Marengo, Dr. Rea, Dr. Hempstead, Dr. Clark, of Rockford, Dr. Hall, Dr. Van Doozer, Dr. McWilliams, Dr. Parsons, Dr. Fenger, Dr. Storck, Dr. Lawless, Dr. Tillosten, Dr. Clendening, Dr. Collins, Dr. Crane, Dr. H. A. Johnson, Dr. Manheimer, Dr. Webb, Dr. Able, Dr. Simons, Dr. McDonald, Dr. Hollister, Dr. Goodall, Dr. R. G. Bogue, Dr. Shepherd, Dr. E. F. Ingals, Dr. A. B. Strong.

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EDITORIAL.

IMPULSIVE AND PARTIAL INSANITY.

The following rule was laid down by the trial judge in the State *v.* Nixon in the district court of Kansas : A person to be held criminally responsible must know the nature of the act, that it was wrong, *and have the will and the mental power to do or not to do it.* The supreme court comments upon this ruling, though it was not directly raised in the points on appeal. The court said, in substance, that it was inexpedient to admit that a person could commit crime of the nature of which he was fully conscious while under the influence of a morbid impulse sufficient to paralyze his

will. The doctrine of "expediency" here affirmed has its prototype in many previous rulings. We doubt if the interests of society can be conserved by the punishment of those who are irresponsible. The question is one as to fact and not expediency. Can a person suffer from disease of the brain, in consequence of which he may commit acts the moral quality of which he fully understands, and which fill him with the greatest distress and abhorrence? We think those of even limited experience in the care of insane will admit the existence of a large number of this class of cases. Many acts of the insane which have little or no ethical significance belong to this category, and we must study the conduct of the insane as a whole. Such being the fact, it is the duty of the courts to admit it and to formulate rules of practice that will conserve society and protect the innocent.

The doctrine of partial insanity, as stated in the same case, that a person may be insane and irresponsible on one subject or set of subjects, and on all others be fully sane and responsible, is much more readily admitted by lawyers than by physicians. It has a certain warrant in the fact that all insanity in which there is not imbecility or complete overthrow of the mind is more or less "partial"; that is, it assumes a certain form and direction, in which the mental impairment is most pronounced. The difficulty lies in deciding regarding that "reasonable doubt" for which the law provides. Can an individual who has suffered impairment of his mental functions to such a degree as to develop delusion, be considered sane beyond a *reasonable doubt* in matters not directly connected with that delusion? We think such a view is hardly in accord with our present knowledge of the physiology and pathology of the brain.

The rule of partial insanity, or monomania as expressed by the court, is of extremely limited and doubtful application. A very few cases that might be included under this head have been reported. The writer in a somewhat extensive experience has seen but one, and that may have been imperfectly diagnosed.

SOCIETY REPORTS.

TRANSACTIONS OF THE CHICAGO GYNÆCOLOGICAL SOCIETY.

LXVI. Meeting Friday, May 28, 1886.

I.—LAWSON TAIT. Abdominal Section for Pelvic Abscess.

II.—NELSON. Specimens Removed from a Case of Supernumerary Digits.

III.—PARKES. Uterine Fibroids Treated by the Fluid Extract of Ergot. (Inaugural Thesis.)

IV.—WAXHAM. Occlusion of the Os Uteri as an Impediment to Labour, with a Report of Two Cases.

The PRESIDENT, DANIEL T. NELSON, M. D., in the Chair.

I.—THE SECRETARY, DR. EDWARD WARREN SAWYER, read the following letter from MR. LAWSON TAIT.

7, THE CRESCENT, BIRMINGHAM, April 14, 1886.

MY DEAR DR. NELSON:—If not too late, I should like to take part in the discussion which was entered into at the Gynæcological Society of your city upon Abdominal Section for Pelvic Abscess. My remarks, of course, are discursive and not very conclusive, because they are based upon only a very few points to which I want to draw attention.

The first is this: I object to the use of words ending in *otomy*, to mean various operations, all of which are practically identical in character but different in detail and not one of which can have any exclusive or absolute identification by any particular name. Thus

Professor Christian Fenger, in the discussion, objects to the use of the word laparotomy, and he introduces another which is perfectly new to me and I hope it will never be used again: it is *oncotomy*. Dr. Fenger objects to laparotomy in a sense where I certainly have no objections, and his very objections only show how utterly absurd all these words are. There really ought not to be any such word as laparotomy in existence, because the signification of its derivatives in the use of the people who spoke the language is such that it could not by any human ingenuity be applied to any modern surgical proceeding. Now the words "abdominal section" are sufficiently English to be understood by everybody, and they are sufficiently distinctive to enable us to understand at once that when they are used the peritoneum is opened. I therefore wish through your powerful society to protest against the use of all these stupid words of Greek formation. I wish also to protest against the absurd distinctions drawn by Snger which are quoted by Dr. Fenger on the subject of pelvic abscess.

He distinguishes six kinds of salpingitis.

1. *Septic*, the existence of which I entirely dispute as a specific ailment.

2. *Tuberculous*, which again I deny except that it has an existence as the third and contracting stage of pyosalpinx.

3. *Syphilitic*, not one particle of evidence of this have I ever seen.

4. *Actino-Mycotic*, which is an equally ridiculous subdivision, based on mere theory, not on fact.

5. *Gonorrhæal*, to which the great bulk of the cases belong.

6. *A mixed form*. Instead of this sixth, or mixed form, I would say that there are a great many cases to which we cannot attribute any actual origin, a number of cases occurring in virgins where the existence of gonorrhœa would be an impossibility, and where there was no puerperal mischief.

Dr. Fenger's paper has always seemed to me to be an illustration of the German savant evolving the descriptions of the camel out of his own consciousness. My descriptions, on the other hand, are taken from some hundreds of cases upon which I have performed operations and the history of which I know as completely as it is possible to obtain information.

In Dr. Reeves Jackson's paper there are two points to which I

want specially to draw attention, and they are not of much importance because they are chiefly questions personal to myself.

The first is a passage in which it is said "Lawson Tait, of Birmingham, and Martin, of Berlin, were the first who attempted to prevent the terrible contingencies of pelvic inflammations by attacking the disease at its original seat; Lawson Tait removed the suppurating uterine appendages, Martin operated for suppurating periuterine hæmatocele. Tait operated for a suppurating hæmatoma of the right Fallopian tube in 1878, and he removed both tubes for pyosalpinx, and an ovary for abscess in 1885. In 1885 Martin performed laparotomy in three cases of intraperitoneal hæmatoma, namely, retrouterine hæmatocele." Now accuracy of date in a matter of this kind is rather important for one's own personal reputation, and Professor Reeves Jackson has underestimated my claim for priority by at least seven years. The first operation which I performed for suppuration of the uterine appendages was done on the 11th of February, 1872, and there will be found in the last edition of my book on "Diseases of the Ovaries," twenty-two cases which I had performed up to the middle of August, 1882, without a death. Since then I have operated upon hundreds. The first case of suppurating hæmatocele which I operated upon is published in detail in the same book; it was in February, 1879, and since then I have operated upon thirty-two cases without a death, and all have been completely cured. It will thus be seen that in none of these matters have the German surgeons approached English surgery as rivals in priority. They have been mere followers in every particular, and I regret to say their following has been practiced without that recognition to which our priority gives us every just claim.

The second point is that in which I find Professor Byford speaking in terms of my own work which no words of mine can sufficiently recognize or express my appreciation of, and here certainly his words of caution are worthy of a little note. What I fear, in fact what I already feel, is that the remarkable success which I have had, and of which Professor Byford speaks in such strong terms, is really leading astray those whose opportunities have not been as my own, into the belief that the work is easy, simple, easily acquired and free from risk. It is not so, and unless those who practice it choose to follow me in the rigid precautions and immense care which

I give, not only to the mere performance of the operation, but to the surroundings of my patients and to every detail in connection with them, they will not obtain, they must not expect, the success which I have had. I have said that I fear, in fact, I already feel, that this success of mine is leading people astray, and I want to urge in the name of humanity, as well as for the sake of the art we practice, that there should be less of the indiscriminate rushing into this kind of work which has been already deplored on both sides of the Atlantic.

I am, etc.,

LAWSON TAIT.

DISCUSSION.

PROFESSOR A. REEVES JACKSON said: We ought, I am sure, to feel honored by having among us in spirit, if not in person, so eminent a man as the writer of this letter. Lawson Tait is in some respects the greatest living surgeon, a Gamaliel at whose feet we all find ourselves sitting; and, withal, a man so observant that not a single gynæcological sparrow falls in any part of the world unnoticed by him. I must plead "not guilty" to the charge—made against me by Mr. Tait—of inaccuracy regarding the date of his first laparotomy for pelvic abscess,—the remarks upon this point having been made by another, and not by me. As stated in the letter, his first operation of this nature was done February 11, 1872, at Birmingham, on a patient of Mr. Hallwright. I am sure that Mr. Tait will not for a moment suppose that any of us would willingly do injustice to one whom we all esteem so highly, and from whom many of us have been recipients of acts of kindness and courtesy.

In regard to the justice of Mr. Tait's criticism on the prevalent use of the words ending in "otomy" I do not feel like being an arbiter. Technical words are frequently necessary, and yet, as a general rule, I think it preferable to use simple language. The ordinary English words are commonly sufficient to answer all the purposes of language. Besides, large and unusual words are sometimes embarrassing. When, some weeks ago, Professor Fenger charged me with performing an "oncotomy" I was afraid that I had done something very dreadful, and the worse because I did it without knowing it. I felt very much as the fisherwoman did when Daniel O'Connell in response to her volley of ordinary, undecorated profanity called her a parallelogram. The fisherwoman did not know what to say, and I could not reply; we both had evidently lost the

thread of the discussion. I am glad that Mr. Tait speaks so strongly in regard to the tendency now so frequently indulged in, to perform laparotomies, and that he is willing to correct to some extent by his words the mischief that has been done by his powerful and successful work. It seems that when some persons visit Mr. Tait and witness his success and simple but effective methods, they come back thinking life is a blank unless they can own and manage an abdominal hospital and spend the remainder of their days in the cheerful occupation of removing uteri and ovaries.

PROFESSOR CHRISTIAN FENGER said:

The letter which Mr. Lawson Tait wrote to Dr. Nelson relates in a number of points to my paper on "Laparotomy for Periuterine Abscess" as well as to some remarks which I made before the SOCIETY in a previous discussion. I must, therefore, beg the FELLOWS of the SOCIETY to bear with me if I take up a little of their time in answering Mr. Tait's letter.

Discussing Professor A. Reeves Jackson's paper, I objected to calling the operation in question a laparotomy. According to the Professor's description of the case, he had opened an abscess which was adherent to the anterior abdominal wall. He had consequently simply performed an oncotomy, an operation which, notwithstanding the division of the abdominal wall, does not differ materially from opening a deep-seated abscess in any other region of the body, as *ex. gr.* in an extremity.

Whether opening the abdominal or peritoneal cavity be termed laparotomy or abdominal section or *Bauchschnitt*, is of course a matter of indifference, provided only that the meaning of the word be agreed upon. There is but one way of getting at the significance of a medical term, and that is by learning in what sense the term is employed in the medical literature of the different nations.

I must again maintain that laparotomy is not merely section of the abdominal parietes, but that the word implies opening of the general peritoneal cavity with a view to performing some operation within that cavity. (See *Linhardt's Operationslehre*. Wien. 1862, p. 705, and *Eulenburg, Realencyclopædie*, Bd. II, p. 37.) French authors occasionally use the word gastrotomy instead of laparotomy. Recently the operation has been called peritonotomy, which on account of its correctness should perhaps be preferred to the other terms.

It is of importance to distinguish between a laparotomy and the evacuation of a limited abscess by simply incising the abdominal wall. The two operations differ widely as to their consequent dangers. Where the general peritoneal cavity is opened, a well-known series of precautionary measures is required before and during the operation, in order to protect the patient from general septic peritonitis.

Where an incision through the abdominal parietes leads directly into a limited abscess-cavity, the precautionary measures essential to laparotomy may be dispensed with; the general peritoneal cavity is not opened and there is no fear of general peritonitis. In the latter operation the peritoneum is not seen or not taken notice of, the incision-wound is left open and the limited cavity washed out and drained.

In every country medical authors hold these two operations apart. Opening a perityphlitic abscess, an adherent hepatic abscess or a parametritic abscess above Poupart's ligament is never spoken of as a laparotomy. Whenever we hear or read of a man having performed a series of laparotomies we naturally suppose that he is experienced in performing intraperitoneal operations. Now, many a surgeon has opened a large number of intra-abdominal abscesses and has never seen the peritoneal cavity.

It is evidently important properly to limit the meaning of the term laparotomy; else we may misunderstand the description of a given case, and the statistics of laparotomies will necessarily be rendered valueless.

The next point Mr. Lawson Tait remarks upon is the question of priority of operating for pelvic hæmatocele.

By the mistake of Mr. Tait, part of what I said in the discussion of Professor Jackson's paper is ascribed to the paper itself. I am thus quoted in his letter:

"Lawson Tait, of Birmingham, and Martin, of Berlin, were the first who attempted to prevent the terrible consequences of pelvic inflammations by attacking the disease at its original seat. Lawson Tait removed the suppurating uterine appendices; Martin operated for suppurating periuterine hæmatocele."

In the discussion I stated the dates at which Tait and Martin performed their respective operations. By a typographical error the dates appeared wrong in some of the copies of the transactions

of the SOCIETY. 1885 instead of 1872 was given as the date of Tait's first laparotomy for abscess of the ovary, and 1885 instead of 1876 as that of Martin's first laparotomy for extraperitoneal hæmatocele.

Considering my remarks as a whole and examining my references to the literature on the subject a careful reader would not have failed to recognize in the wrongly printed dates a typographical error.

Lawson Tait performed his first laparotomy for extraperitoneal hæmatocele in February, 1879. (See LAWSON TAIT: *Diseases of the Ovaries*. 4th Edition. London, 1883, p. 346, and Tait's letter.)

Martin performed the same operation for the first time in 1877, and a second time on the 31st of January, 1879. (See A. MARTIN: *Das extraperitoneale periuterine Hæmatom*. *Zeitschrift für Geburtshilfe und Gynäkologie*. VIII Bd., Hft. II, 1882, p. 476.)

It will thus be seen that my statements concerning priority were correct. In the discussion of Professor Jackson's paper I quoted Säger's classification of pelvic inflammations. In criticizing it, Mr. Lawson Tait speaks of "absurd distinctions." I wish to repeat that I regard Säger's classification as complete and correct and in accordance with the laws governing inflammatory processes in all organs of the body.

To Mr. Lawson Tait's manner of criticizing my little paper "On Chronic Periuterine Abscess and its Treatment by Laparotomy," which appeared in the May number of the *Annals of Surgery*, 1885, there is but one answer.

Like all published articles it is open to criticism. If anybody should wish to attack it, I am ready to enter into a discussion of it, provided that tangible objections to it be brought forward.

II.—The PRESIDENT exhibited specimens removed from

A CASE OF SUPERNUMERARY DIGITS.

He said: While I know the condition is not exceedingly rare, I thought the specimen was so beautiful as to be worthy of presentation to the SOCIETY. The specimen consists simply of two supernumerary little fingers, which I found in a beautiful, healthy baby just after it was born, attached by small pedicles, consisting simply of the skin and the vessels needed to supply them, about the middle of the first phalanx of the little finger; the pedicles were perhaps one-sixteenth of an inch in length, just long enough to ligature. They look like little beans; the finger nails are fairly developed in both.

They were very vascular. They looked, before removal, like bangles. This was the sixth pregnancy; the other children were all perfect; no other case of this condition in the family that is known. The condition is usually hereditary. In one there is a very good nail formed, upon the other there is only a slight nail. The mother is in good vigor and health. They were united to the larger little finger about the middle of the first phalanx—one was just about the middle of the phalanx, on the outer border; the other, half way between the middle line and the outer border. They both feel as if there are bones in them—two phalanges in each, the third being represented by the pedicle.

III.—PROFESSOR CHARLES T. PARKES (Rush, 1868,) read the following paper, entitled

UTERINE FIBROIDS TREATED BY THE FLUID EXTRACT OF ERGOT.

My intention is to relate to you the history of four cases of uterine tumor, and to present a few remarks suggested by them. These four cases were treated by the internal administration of Squibb's fluid extract of ergot. They all resulted in recovery by expulsion of the growth.

I found no insurmountable difficulty in giving the medicine, although when given for a prolonged period it creates nausea and disgust in some. This was counteracted, and the pain following its use controlled by combining it with morphine. It seemed to me preferable to the hypodermatic use—the latter being locally painful and often producing abscess, besides it is not followed by any better result. Two of the cases, treated by ergot, when thrown off, proved to be pure uterine fibroma—dense and hard—white and glistening when cut open—consisting of simple fibrous tissue. The other two following the action of ergot were soft myomata—pulsa-ceous and semi-elastic—consisting mostly of connective tissue, confirming the diagnosis made. All four of these were evidently sub-mucous tumors, or so slightly interstitial as to be practically covered only by mucous membrane.

CASE I.—MRS. S., American, 43 years old, widow, three children, no miscarriages, menstruated first when 16 years old. Never had any noticeable trouble with menstruation until three years previous to my first examination; during these years she had suffered with irregular profuse hæmorrhages which were now continuous, accompanied with exacerbations on the slightest exertion. My first ex-

amination was made February 20, 1876. As my memory brings this patient before me she presented the most perfect example of transparent flesh that I had ever seen. A large, finely formed woman, her flesh looked like alabaster, apparently destitute of blood. The legs were œdematous, the heart beat feeble and rapid, and the slightest exertion was followed by extreme palpitation and the most fearful feelings of suffocation. Her answer as to what she had done for her trouble was that she had taken "quarts of medicine." Vaginal examination revealed an enlarged uterus and patulous os, from which blood was rather freely oozing. The sound entered the uterus about five inches, the handle being deviated forward and to the left side. A diagnosis of submucous uterine fibroid was made.

The treatment adopted was the administration of strychnia and iron, together with wine and good diet for the general condition, and one-half drachm doses of Squibb's fluid extract of ergot every six hours, to either expel or kill the growth. Locally, to stay the hæmorrhage, a small tampon of pulverized alum was applied to the *os uteri* and held in position by ordinary cotton tampons.

The first forty-eight hours' use of the ergot produced quite severe uterine pains, so acute that the patient, in her weakened condition, said they were unbearable. At this visit Professor T. D. Fitch was with me in consultation. The tampon of alum was removed, and Dr. Fitch's examination confirmed the diagnosis made, and advised the continuation of the treatment. As the bleeding had been entirely controlled by the tampon, it was left out. The ergot was continued as before, and a sufficient dosage of morphine ordered, to make the pains bearable if they persisted. No further use of the tampon was required, the uterine contractions never ceased while the ergot was administered. On the sixth day of its use a foul-smelling serous discharge came on, *per vaginam*, accompanied with slight general chilliness and a temperature of 102° F. The patient was assured that the tumor was surely coming away, and encouraged to bear "yet awhile" with her great suffering. On the eighth day the tumor was found in the vagina and removed. It was about the size of a duck's egg and very hard to the touch. After a short period of mild septic trouble the patient passed through a quick convalescence and rapidly recovered. Seen a few months ago she says she has never had any illness since getting rid of this growth, and certainly looks well.

CASE II.—Mrs. P., German, married, five children, no miscarriages, menstruation began at 14, now 37 years old. Seen by myself first in March, 1881. The patient is a robust, hearty woman. Never had any trouble until six months after the birth of last child, about one year ago, when she began to flow too freely and too often—as often as every week or occasionally twice a week. The blood was in large quantity and bright red in color.

The examination revealed an enlarged uterus—it could be felt above the pubis during bimanual examination. The sound entered easily for five inches, the handle deviating forward and to the left. Its use was accompanied and followed by very free bleeding.

Diagnosis.—Submucous uterine fibroid on anterior wall. *Treatment.*—locally the alum tampon was used as in the previous case. Squibb's fluid extract of ergot in half drachm doses every six hours. The patient was ordered to remain in bed. On the succeeding day all the tampon, except the alum, was removed. No hæmorrhage; slight pains complained of. On the second day the pains were very severe and morphine was given to control them. The alum tampon was removed and a carbolized hot water injection ordered three times a day. On the third day pains still severe and a foul-smelling vaginal discharge commencing.

This condition persisted until during the night of the eighth day, when I was summoned to the patient on account of the unusual severity of her sufferings, the messenger, her husband, saying it was just as if she were having a baby. On my arrival the pains had quite ceased. Examination showed the loss of considerable blood, and the vagina was found filled with a large fleshy mass, horribly offensive. The finger could be passed beyond it, and the largely opened cervix recognized. It was seized with a Vulsellum forceps, twisted a few times upon itself, and then delivered. The mass was as large as a closed fist, dark colored and ragged all over its surface and very foul smelling. The patient rapidly regained her usual health, and is well today.

CASE III.—Mrs. E., 33 years old, married, three children living, one miscarriage, menstruation commenced when 14 years of age. Was first called to visit her January 2, 1885, for severe uterine hæmorrhage. She then informed me that she never had any trouble with menstruation until about two years previous.

Shortly thereafter she was operated upon for laceration of the

cervix, without much relief to her trouble, since she had gradually grown worse, so that she was not free from bleeding ten days in the month.

One year previous to my seeing her, the uterus was freely curetted and fuming nitric acid applied to the cavity as a relief for the bleeding. The procedure failed in any good result.

At this visit the bleeding was extreme in degree. Examination revealed the pelvis largely filled with a smooth, doughy mass. After considerable searching the *os uteri* was found high up above and close to the pubis. It could only be found by crowding the finger between the bone and the growth. The growth was exquisitely tender to the touch or any manipulation. Bimanual palpation discovered an uncertain mass above the pubis. The vagina was tamponed temporarily, and morphine administered hypodermatically. The diagnosis was reserved. In my mind it rested between hæmatocele and soft myoma. The tumor was compressible, at least its elements seemed to give way to the pressure of the finger; it was semi-elastic and painful under manipulation, filled the entire posterior half of the pelvis, and the *os* was carried well upward and forward. It might be, and probably was, a myoma of the posterior uterine wall retroverted.

(A few weeks ago I had the satisfaction of seeing the *fac simile* of this case, so far as the character of displacement and the position of the tumor was concerned, in a patient under the care of Dr. Merri-man, although the tumor in Dr. Merriman's case was much harder and more resistant.

Dr. Merriman, with skill and apparently with ease, lifted the tumor out of the pelvis into the general abdominal cavity, after placing the woman in the knee-elbow position—a change bringing much comfort to the patient. I learn that under the use of ergot the growth is already diminishing in size.)

The patient was put upon fluid extract of ergot in one-half drachm doses three times a day. The next menstrual period showed no change other than a diminished loss of blood. In June the flooding was quite free and accompanied with considerable pain. In July everything was as bad as possible, with so much pain that the ergot was discontinued. Repeated examination now narrowed the diagnosis down to soft myoma. The removal of the uterine appendages was suggested, in the hope that this procedure would anticipate the

menopause, stop the bleeding and lead to the gradual atrophy of the growth. In September, consultation was solicited with Professor W. H. Byford, when the patient was etherized and carefully examined. The sound, introduced with great difficulty, owing to the displaced position of the os, passed in over five inches, positively demonstrating the nature of the growth, its consistency showing it to be the soft variety of myoma.

As the patient could not be said to be in absolute danger of her life, the operation was refused by her friends, although the sufferer was willing enough to have it done.

The previous treatment was endorsed by Professor Byford, and its continuance advised.

The fluid extract of ergot was resumed and rendered bearable by morphine.

The October illness was accompanied by slight hæmorrhage, but excessive pain. These uterine contractions continued on after the menstruation ceased, until, during the last week of October, they became labour-like in character. Examination now revealed that the uterus had righted itself, the os was becoming patulous and its edges thinned out; through its opening the projecting tumor could be felt. On the second day of November, pieces of the broken-down mass, horribly offensive, could be seized with the forceps, pulled out of the uterus and cut away.

Chilly sensations began to be felt by the patient, sweatings came on, the temperature ran up to 101° F., and a mild septicæmia was established. During the following week the pains never ceased. Quinia was administered freely. The vagina and uterus were irrigated with hot carbolized injections, and the mass removed as fast as any of it could be reached, and at the week's end the last remnant was gotten away. The patient was very much reduced physically, but rapidly convalesced, and is now perfectly well, with her menstruation normally re-established. Fully a quart of soft pultaceous pieces of the growth were removed.

CASE IV.—Mrs. L., German, 34 years old, one child, was seen first November, 1885. She had been suffering with increased menstrual flow for a year. She came to me to be treated for an external, painful, labial swelling. It proved to be a vulvo-vaginal abscess. It was opened freely and gave no further trouble. A uterine tumor was noticed and examined. It was found to be of considerable size,—could be detected above the pubis.

She was put upon one-half drachm doses of fluid extract of ergot every six hours.

This she continued for six months steadily, with varying conditions of pain and hæmorrhage, until in April, 1886, the hæmorrhage ceased, pain became very severe, and a shreddy, foul-smelling discharge manifested itself. She was removed to the St. Joseph's Hospital, and after ten days of antiseptic washings and removal of masses of broken-down tissue, the mass was entirely extruded. This patient had also quite a severe septicæmia, but finally recovered and is now well.

Remarks:—Aside from crucial demonstration, it seems reasonable to assert that these four cases were cured by means of the remedy used, and by that alone. It is well known that the most of authorities state that no positive reliance can be placed on the use of ergot. My experience surely, as here illustrated, and confirmed by other cases seen, leads me to think that this adverse judgment must be qualified, especially in the treatment of the sub-mucous bleeding fibroids of the uterus. If the growths be partially interstitial, the less the thickness of uterine tissue between them and the mucous covering, the more certain will the remedy be curative. Of course it will be impossible to demonstrate the exact amount of uterine wall forced contractions will destroy, hence a trial of its worth is desirable in all cases not purely sub-peritoneal and pedunculated. Still I am quite convinced the severity of the hæmorrhage gives one a good reason to speak *positively* of the results to be accomplished by its use. I have not deemed it necessary to look up the history of the first use of this remedy for stimulating uterine contractions. It is sufficient for me to say that my confidence in the remedy and persistence in its use, even when failure of good results seemed certain, has followed directly as the result of the teachings and experience of an honored Fellow of this SOCIETY, Professor Wm. H. Byford, who has never lost an opportunity to urge upon the profession his belief in the specific action of the remedy, and its absolute certainty of cure in many cases. I am quite sure that Professor Byford deserves the credit of being the first to make use of ergot with the idea of destroying the vitality of the growth, and as well, causing its expulsion from the uterus.

The third case shows plainly how absolutely unnecessary any

operation would have been. The removal of the appendages might have stopped the hæmorrhage, but such a perfect cure as now exists could never have followed operation, to say nothing of the harm done by unsexing the woman; still no case could present better reasons for such a procedure, none in which it would have been more justifiable from the indications present. To me it brings the lesson to make oöphorectomy the *dernier ressort* in all cases, certainly to give the remedy used at least a six months' trial without result before operation be sanctioned.

The difficulties attending the differentiation between a distended sack of fluid and the soft myomata was well illustrated by this case. The sensation communicated to the touch was scarcely distinguishable from fluctuation. It was only after repeated examinations under ether, and the use of the sound, that the diagnosis was satisfactorily settled.

The fourth case for a time seemed one in which the treatment would come to nothing. Every one became discouraged. The suffering was increased, and no advance was made, apparently. By persistence the cure was accomplished. In this case operative interference was solicited by the patient, and would have been most readily submitted to, without any urging. If I read aright the indications which authorities give, to justify the resort to removal of the uterine appendages, they were all present in this case, and more too if that were needed. Certainly the final result has proven any such interference would have been uncalled for and lamentable.

I am quite well aware that four cases cannot be considered absolutely demonstrative of any rule, still these four increase the number already published in proof of the curative action of ergot, administered thoroughly, for sub-mucous uterine growths. It is impossible for me to understand how some good authorities can still assert their disbelief in ergot; in fact, calling it the most inert and disappointing of all drugs. No possible argument can disabuse my mind of the belief that its action was positive and certain in the cases related. No law has as yet been evolved fixing even by approximation the period of time required for the effects of the medicine to show themselves. The idiosyncrasies of the patient, the thickness of the uterine envelope, the distance from the mucous membrane, the purity of the drug, and many other conditions,

render it scarcely possible that any such law can ever be laid down. The trial should be made patiently and persistently, just so long as the patient's condition will warrant its continuance, and a complete expulsion of the growth, followed by rapid recovery in many cases, will be the reward.

IV.—PROFESSOR F. E. WAXHAM read the following paper, entitled
OCCLUSION OF THE OS UTERI AS AN IMPEDIMENT TO LABOUR, WITH
A REPORT OF TWO CASES.

Having met with occlusion of the os but once in several hundred cases of labour, and knowing of a number of physicians of extensive practice who have never seen this condition present at the time of confinement, I am convinced that it must be of rare occurrence, and the history of two cases may not be uninteresting.

Mrs. S., primipara, twenty-nine years old, German, fell in labour about 9 P. M., February 21, 1885. The membranes ruptured soon after the commencement of labour and the amniotic fluid gradually drained away.

The patient was seen between 3 and 4 A. M., at which time the pains had become very severe and frequent. Upon examination the head was found low down in the inferior strait, almost presenting at the vulva, and covered apparently by a thin membrane through which the advancing head threatened to burst with every pain. Upon the most careful digital examination, no os could be discovered nor the slightest indication of one. Professor Nelson was summoned and promptly responded. His more experienced finger detected a very slight dimple in the center of the presenting tissues. By keeping the finger upon this slightly thickened tissue, he discovered that it became very much thinner with every pain, while as the pain subsided the tissues assumed a very slightly umbilicated appearance. By firm and continued pressure upon this suspicious spot an opening was at length effected and the os gradually dilated. As the labour proceeded slowly, and fearing the result to the child of so long a delay of the head in the pelvis, and the os being fully dilated, the forceps were applied. The child was delivered without injury to the mother, but it was asphyxiated and required considerable effort in resuscitation. Professor Nelson stated that this was the second case that had ever come under his observation, and kindly gave me the history of the following one. He was called to attend a lady in her first confinement, a Swede, twenty-three years old, and

married about one year. Making a hasty examination, he found a well-formed cervix but did not detect the os. On returning a few hours later, the head had descended to the inferior strait and was indeed presenting at the vulva and covered by the cervix, which had become so thin as to resemble the membranes. The membranes had already ruptured and the amniotic fluid had gradually escaped. There was no appearance whatever of the os. It could not be detected with the finger, and the head seemed about to burst through the uterine tissue. The patient was placed before a window, the labia separated and careful search made for the os. Only after a most careful search was it found. It was patulous only to the extent of admitting the very finest surgeon's probe. After this had been introduced and worked about, a second probe was passed, and by separating them the os was gradually and sufficiently dilated to allow the finger to enter. The os was then rapidly dilated and labour progressed normally.

I find the literature on this subject quite meager, many of our writers on obstetrics omitting the subject entirely, while others refer to it very briefly.

Schroeder alludes to it in the following terms :

"As complete atresia of the os prevents conception, it follows that an occlusion of the os, observed in labour, must have taken place during pregnancy.

"Very frequently there is a superficial and easily separable agglutination of the external os. It is due to an inflammatory process of the lips of the os from a previous blennorrhœa. During labour the advancing head is seen to push the lower uterine segment forward to the outlet, and to thin it more and more. This thinning may be so great that the head appears to be covered only by the membranes. By an accurate examination the os feels like a small and soft dimple directed greatly backwards. If during a pain the finger or uterine sound be forcibly pressed against the dimple, the agglutination of the os will suddenly give way. The os itself now very rapidly dilates and labour proceeds without impediment. Often the pains themselves succeed in breaking down the adhesions of the os.

"It very rarely happens that the os only partially dilates after the agglutination has been torn through and remains rigid so as later to require incisions. There is very seldom so firm an adhesion between the maternal and fetal membranes in the immediate vicinity of the internal os that the lower uterine segment cannot retract over the ovum. Separation by the finger or rupture of the membranes renders possible the dilation of the os."

Schroeder also refers to the fact that a firm cicatricial band may occasionally occlude the os, resulting from inflammation of the cervix or cauterization :

When these firm adhesive bands prevent dilation of the os there is danger of rupture of the vault of the vagina unless incisions are made and assistance given. The cicatricial closure of the os, is frequently incomplete; more or less fine openings remaining pervious, rendering conception difficult but still possible, is believed by Schroeder to frequently result from ulcerative inflammation during the lying-in state.

Leishman, in discussing this subject, remarks that

"There are some cases in which there seems to be actual occlusion of the os. Impregnation in the case of an absolutely occluded os is as impossible as that the normal function of menstruation should be carried on, and therefore we must assume, in such cases, that the closure must have taken place subsequently to the entrance of the seminal fluid. It is of course possible that the os may remain open to a very limited extent, and yet the state of the tissues renders distension impossible, so as practically to constitute an impediment as insurmountable as actual occlusion would be."

Playfair gives the following brief intention of this condition :

"Agglutination of the margins of the os uteri is occasionally met with and must of course have occurred after conception. It is generally the result of some inflammatory affection of the cervix during the early months of pregnancy. Usually it is not associated with any rigidity or hardness, but the entire cervix is stretched over the presenting part and forms a smooth covering in which the os exists only as a small dimple and may be very difficult to detect at all. Occlusion of the os from inflammatory changes sometimes so alters the cervix that no sign of the original opening can be discovered."

All our authorities agree that the occlusion of the os is the result of inflammatory change occurring subsequent to impregnation. It is a noteworthy fact that in both these cases the membranes ruptured and the amniotic fluid escaped in the very early stages of labour, showing that the membranes were adherent to the uterine tissue about the internal os. As the internal os dilated, rupture of the adhesions and of the membranes necessarily followed.

The discussion of the papers read by Professor Parkes and Professor Waxham was, on motion, deferred until the June meeting.

Mr. Lawson Tait, of Birmingham, and Professor T. Gaillard Thomas, of New York, were elected Honorary Fellows of the Society.

Professor E. C. Dudley proposed for honorary fellowship Protheroe Smith, M. D., M. R. C. P., of London.

W. W. JAGGARD, M. D.,

Editor.

July 10th, 1886.
2330 Indiana avenue.

CHICAGO MEDICAL SOCIETY.

Meeting, June 7, 1886.—THE PRESIDENT, E. J. DOERING, M. D., in the chair.

PROFESSOR A. REEVES JACKSON read a paper entitled
THE INTRA-UTERINE STEM IN THE TREATMENT OF
FLEXIONS,

exhibiting the stems used. The essayist began treating uterine flexions with the stem pessary in 1870. Prior to that time the only methods he had employed were gradual dilatation and incisions. The results were so unsatisfactory that he sought for a safer and more successful method. Having received the impression that the use of the stem pessary was more hazardous than either the dilating or cutting plans, he commenced its employment with misgiving, and did not rely wholly upon it, but preceded it with either gradual stretching or slight incisions. In two cases this mixed method was followed by pelvic abscess, a sequence which he had never observed when the stem alone had been used. All cases of uterine flexion are not accompanied by dysmenorrhœa or sterility, yet when there exists a relationship between these symptoms and an existing flexion the latter must be looked upon as a mischievous factor and one that should be removed. He had never treated any case of flexion in which dysmenorrhœa was not present, although coexistent barrenness has been frequently an additional incentive to the patient to undergo efforts at cure.

He preferred Chambers' bifurcated vulcanite instrument, although the divergence of the branches *below* the internal os uteri was a radical defect in the instrument as ordinarily

used. Frequently the branches should be closed so that the stem might be practically single in that portion which traverses the cervix. His method is as follows: A flexion and its direction being diagnosticated, a flexible bougie is passed through the bent portion of the canal and quite to the fundus. The depth of the canal being carefully noted, a pliable stem, consisting of the distal portion of the same, or a similar bougie, one-third of an inch shorter than the ascertained depth of the canal, is selected for introduction. A flange or bulb is formed upon the outer end of the stem by rolling upon it a section of rubber tubing. The woman being placed on the back, in Simon's position, and the os uteri exposed with a speculum, the stem, either grasped with a dressing forceps or mounted upon the end of a piece of pointed wire, is passed entirely into the uterus. A large tampon of cotton moistened with slightly alumized glycerine is pressed against the bulb of the stem, and allowed to remain one or two days. The tampon is removed and replaced at suitable intervals, until the tendency of the stem to leave its position disappears. After this yielding stem has remained from one to three weeks, according to the degree of tolerance manifested by the uterus, it is removed and a thicker one put in its place. This likewise is permitted to remain a week or two, and is then replaced by a Chambers stem. While not very much, or, indeed, any change of shape is to be expected in consequence of the use of the flexible stem, yet, in several instances, a very considerable alteration took place within a few weeks, or even a few days, and in a few cases it was found unnecessary to resort to a rigid instrument at all. Usually, however, it had been necessary to use an inflexible instrument for from three months

to a year—not continuously, but for periods of three or four months, with an interval of a week or two, during which the stem was removed in order to test the degree and permanence of the improvement. The feature of this treatment which is essential to its safety and success is its slow and gradual conduct, and the non-observance of this necessity has been the cause of dangerous results and failures to cure.

The drawbacks attending this method of treatment were :

1. Difficulty in retaining the instrument in position ;
2. Pain ;
3. Hæmorrhage ;
4. Pelvic inflammation—all except the first being common to all other methods of treatment.

A table comprising the details of sixty-four cases treated by the intra-uterine stem alone was given, showing the ages and social conditions of the patients, the direction of the flexion and the result of the treatment. Of the entire number, 42 occurred in married and 22 in single women. Of the former, 8 had borne children ; the other 34 were sterile. Of the latter, 8 subsequently bore children. A cure of the flexion followed in 40 ; of the remaining 24, four were improved and relieved of dysmenorrhœa. In 20 the result was unknown. The ages of the patients ranged from 19 to 39 years. The uterus was anteflexed in 50 and retroflexed in 14.

In conclusion, the author said : “I believe the principle of the intra-uterine stem in the treatment of flexions to be correct ; and it need not be dangerous—at least, no more dangerous than any other effective method. I further believe that by its use more cases of uterine flexion can be cured than by any other means at present in vogue. The conditions of both safety and success are watchfulness, patience and slow progress.”

PROFESSOR D. T. NELSON, in opening the discussion, said: Mr. President, I am glad to have heard the paper, and think it is a most valuable one. The cautions that it gives are certainly those that all of us should remember, to wit: the length of the instrument used compared with the length of the uterus, the slow and gradual dilatation of the uterus before using the inflexible stem, and removing it on the occurrence of bad symptoms. In recent years I have not been in the habit of using the stem pessary as much as my friend Professor Jackson, but I think that with his present instructions I shall try it again. Not that I have not tried gradual dilatation, and the gradual, slow, careful straightening of the uterus, but I have not by this particular means caused the pessary to be retained as constantly as he has. The vulcanite pessary, and the various other forms, including the Wright or Chambers modification, I have used, and with many of the difficulties the doctor has narrated. But with his modification it seems to me very likely we can use them with better success. The irritation produced by them has been a great drawback, and in recent years it has been my habit rather to use the form of pessary recommended by one of our members, Professor W. H. Byford, the slippery elm bougie. It produces a gradual dilatation of the uterus, and often produces remarkable results in the treatment of the flexions, and I have had no bad results from its use. One point that the doctor did not emphasize sufficiently is that the instrument should not be retained long if it produces pain, but it should be removed and the patient put in bed. I should have preferred to have him give directions for the patient to remove the instrument if the pain continued for a long time; for if it does, the instrument ought to be removed, and if he should happen to be out of the city and the patient should be unwilling for anyone else to see her, serious

disease might commence before he returned and removed the instrument. For this reason it is, and always has been, my plan to have the instrument so arranged, by a string or something of that sort, that the patient can remove it herself. We should remember that the instrument should be less than the uterus by a third of an inch; that the uterus is to be put into its proper shape, in a splint, as it were, and then expected to grow right; that it is not cured when it is straightened; if it has been displaced for a considerable time there has been an atrophy of the uterine tissue on one side, and it may take weeks, or perhaps months, to alter the nutrition of the different parts of the organ, and until that change has taken place it is not likely that the patient is permanently cured, unless pregnancy has taken place and altered the nutrition of the parts. As to pelvic inflammation, the author has been more fortunate than most of us in the use of stem instruments. One point I wish to add, viz.: That when there is any possibility of gonorrhœal poison lurking in the genital passages of the female, greater care should be taken in the use of such instruments, or operative procedure of any sort, for that matter. I feel, when there is reason to suspect that this poison has once been implanted, that I hardly dare to introduce sound, pessary or other instrument in the interior of the uterus, and believe that such an instrument should be used with the greatest caution in these cases.

PROFESSOR E. C. DUDLEY said: Mr. President: The marvelous freedom from dangerous inflammation following the treatment of uterine flexure by forcible dilatation and by the application of the intra-uterine stem, furnishes a striking illustration of the fact that the human uterus will sometimes endure an immense amount of abuse. My own preference is generally for the former method, as advocated by Goodell,

Ellinger and others. My experience has only tended to confirm me in the impression that forcible dilatation is reasonably satisfactory in its results, and that the results are reasonably permanent. I would seldom advocate the use of intra-uterine stem pessaries for retroflexion unless the flexure were of the so-called congenital variety, and therefore associated with atrophy of the uterus, a condition which is very rare. The essayist has, perhaps for reasons of brevity, omitted to make the distinction between physiological and pathological antelexion. This distinction within a few years has been quite clearly defined by Schultze, Fritsche and others, and their teachings are now recognized as correct by many of the leading gynæcologists throughout the world. In the light of their investigations the old diagram of Kolrausch, which for more than twenty years has generally formed the basis for the illustrations of the normal position of the uterus, is now quite generally discarded. The uterus has no absolutely fixed position, but it has a certain normal range of movements. The angle between the body and the cervix may vary, according to the varying quantity of material in the rectum and bladder, from zero to at least 45° ; Fritsche says 90° , and his observation is possibly within the physiological limits. When the bladder is full the uterus becomes straight and the angle of flexure disappears. When empty, the angle may measure from 45° to 90° , and yet not be pathological. It is, moreover, probable that a flexure of much less than 45° when the bladder is empty should be considered pathological. Furthermore, antelexion, even within the defined limits, is always pathological if there be immobility at the angle of flexure; indeed, a displacement exists whenever the organ is restrained from its normal movements. In a

word, antelexion is pathological if the mobility at the angle of flexure be increased or decreased beyond the physiological limits, or absent. Want of a clear understanding of these simple facts has led to the invention of innumerable pessaries for straightening the antelexed uterus, and they have been persistently employed, to the detriment of the patient, in cases of perfectly physiological antelexion. Suppose a case : The uterus is shown by digital examination to be so low in the pelvis that when the bladder is empty its entire anterior wall is easily touched. The physiological flexure, which may be from 45° to 90° , is then perfectly apparent to the examining finger ; the symptoms of vesical irritation are attributed to the flexure, and an antelexion pessary is accordingly introduced which produces pressure upon the anterior wall of the uterus. The symptoms disappear, and the conclusion is erroneously formed that the relief was dependent upon the straightening of the uterus, when in reality the pessary has, perhaps, produced no such effect, but has merely lifted the uterus to its health level, and thereby relieved the symptoms, which were due not to flexure but to descent. The same manner of treatment has often been followed by relief from similar symptoms attributed to anteversion, when in reality the pessary, by lifting the cervix to a higher level, has exaggerated rather than reduced the anteversion. For this reason all vaginal pessaries especially designed for anterior displacements are in no respect superior to the ordinary Hodge pessary ; indeed, they are objectional, because in overcoming the descent they press upon the uterine wall and thereby cause irritation of the organ. Antelexion is only a symptom which may result from any one of a variety of widely different causes, such as adhesions,

uterine fibroid, parametritis posterior, or failure of the puerile uterus to develop at puberty. It would indeed be irrational to attempt the relief of a symptom due to such diverse causes by any single plan of treatment. The essayist would not attempt to do this, but he has neglected to specify the particular flexures for the relief of which he deems the stem applicable. Inasmuch as many of these flexures are dependent upon the uterine or peri-uterine inflammation, and inasmuch as there is reason to conclude that dysmenorrhœa and other evils are more the result of the inflammatory state than of the flexure itself, I would advise that the stem be reserved for cases which are not relieved after the inflammation has been removed by safer methods. Such a plan would certainly restrict the use of the stem to a very small number of cases, because the symptoms for which it is to be employed would so often disappear upon the cure of the inflammation. It is indeed probable that the dysmenorrhœa for which the author has employed the intra-uterine stem may depend rather upon some faulty nutrition, or upon some disease of the uterus independent of the flexure, and that the stem therefore gives relief by some change which it produces in the nutrition of the organ. If this be true, it would then follow that anteflexion *per se* really furnishes no positive indication either in itself or in its results, but that the same treatment would be equally effective under similar conditions without the co-existing flexure. Congenital anteflexion of the puerile uterus is undoubtedly a condition for which the stem may be considered one of the legitimate means of treatment. Sterility, whether associated with pathological flexure or not, has been successfully treated by the stem. Winckel says that the presence of the

instrument may give a better development to the menstrual decidua and thereby make a better bed for the ovum. One objection to the stem, strongly urged by Schultze, is that by its use the physiological flexure is overcome, and it therefore may be said to produce, rather than to relieve, displacement. But we should not permit theoretical considerations to bias our judgment in face of the author's carefully observed results. His contribution is certainly a valuable one, and shows that the instrument, at least in careful hands, is less dangerous than is ordinarily supposed. The author's freedom from inflammatory results is doubtless due to his judicious preparation of each case by means of the olive-tipped bougie. Undoubtedly the observations of Professor Jackson and others must be considered as placing the intra-uterine stem among the useful and approved resources in the treatment of these troublesome cases, but even at the risk of prolixity I again protest against the indiscriminate treatment of purely physiological ante flexion by any means soever.

DR. H. P. MERRIMAN said: I have very little to add to what has been said. The use of the various methods that have been proposed seem to me to aim at one given end,—to change the nutrition of the uterus. Forcible dilatation does that to a certain extent; it is temporary, however, in its action. Incision produces an alterative effect and accomplishes its purposes. It does not succeed a great many times, neither does the temporary action of dilatation. The use of the stem pessary, on the other hand, succeeds because it is keeping up a continuous pressure upon the parts. Now I am decidedly in favor of this treatment by stem pessary; it strikes me that it is the only rational method of treating these flexions, which are pathologi-

cal. After the cause of a flexion has been removed,—that is, the inflammation of the uterus, or the pressure of a tumor, or pressure of heavy clothing, or whatsoever causes it,—the uterus does not always return to its natural state, and then we need to introduce some method for restoring it to its normal condition, and I do not know any more rational method than this one. This paper strikes me as a very valuable one. The valuable part of this treatment seems to me not to be so much in the use of this stiff stem as the earlier treatment by the flexible stem, where, by continuous pressure upon the parts, we are able to accomplish the same effect as passing a sound in chronic cases of gleet, producing a healthy action in a diseased organ and thus producing absorption of a pathological exudate. It strikes me that the doctor recognizes this condition, for often before using the stiff Chambers stem when he has been using these bougies, in a great many instances he has found the treatment has nearly cured the disease, and if it had been continued longer I believe a cure would have been effected. The intra-uterine stem, by continuous pressure, induces an alterative action of the tissues, the absorption of exudates and a gradual return to the normal condition of the uterus, and a natural tendency toward a straightening of the uterine canal as the uterus becomes healthy.

PROFESSOR SARAH H. STEVENSON said: I have listened to the paper with a great deal of interest, and also to the discussion. My methods are different. I have used the stem pessary a great deal in former years, but for the past two years I have discarded it entirely as some of the results were unfortunate, although I think I have never had any serious results from the use of the stem. I now use, and have for the past two years, the galvanic current entirely, and it is applicable to all cases, especially in those in which the stenosis is so great as not to

admit the passage of the bougie. I have never found a case in which I could not use this method with satisfactory results.

DR. H. T. BYFORD said: I quite agree with Dr. Dudley in his trite but very true remark, "It is wonderful what an amount of abuse the uterus will stand," and I congratulate Dr. Jackson that he has discarded incision and dilatation in treating flexions. I also congratulate him upon his good success. I believe the mortality from this treatment—the treatment by the intra-uterine stem—has been estimated to be from $\frac{1}{2}$ to 1 per cent. by those who have investigated heretofore. Whether it is so now I do not know. The present per cent. of inflammation of the cellular tissue varies from 2 to 5 per cent., as nearly as I can determine. There are an immense number of cases in which the stem caused inflammation, which have never been published. It seems to me that in considering this subject the reason for this treatment should be made more apparent. There are some who use it as a splint or merely to straighten the uterus; others use it as a stimulant on account of its continuous pressure. There is no doubt it stimulates and temporarily straightens the uterus, but it is well known that in time, in a large proportion of these cases, the uterus again becomes flexed. The question arises, should we try to straighten the uterus? As Drs. Dudley and Schultze have said certain flexions are supposed to be physiological (which I don't believe), the uterus is supported in the neighborhood of the internal os, which may be said to have a fixed place in the pelvis. The elasticity of the tissue will allow that part of the uterus to be pressed in nearly all directions, but it will come back. The fundus may bend forward or backward and remain in such position for some time, and the uterus still be in a normal position. During youth the child who sits too much, has a curved spine, etc., having a uterus pretty firmly

fixed at the cervix, will often have the uterus pressed upon by the abdominal contents in the wrong direction. The normal resistance of the uterus to flexure will be gradually overcome (the uterus may even become atrophied), and a flexion results which, when slight, may be called a physiological flexion, and may exist without causing trouble; but it is pathological. The elasticity which the uterus of normally firm structure displays during the filling up and emptying of the rectum and bladder is hardly worthy of the name of flexure. Any considerable permanent flexure occurring in this way must be the result of want of firmness in the structure of the uterus. If we are going to use a supporter we should use it when the flexure is forming, not after it has been produced. If we will use such treatment as will remove the improper pressure upon the uterus, viz., by straightening up the spine, using exercise, etc., etc., a stem will be seldom necessary, because whatever flexion has already been produced will usually not cause unpleasant symptoms. If it has gone to the degree of producing atrophy of the uterus we may need to use a stem pessary, but as a stimulant to the uterine tissue rather than a straightener of this organ. I have seen uteri bent almost like a horseshoe become impregnated and return almost to their former degree of flexion. The intra-uterine stem, in view of its slight action as a support and powerful action as a stimulant, and its notoriously bad record, should be the last resort. The frequency with which Winckel uses the stem is now about once in 218 cases, while formerly he used it about once in 50, and he is using it less all the time. In my experience and the experience of a great many others, if we cure the acute or subacute inflammation of the uterus and then apply stimulating measures we nearly always accomplish the cure of the flexion by safer means. There are, of course, a few cases left in which

the use of the uterine stem may be justifiable, but I think they are exceedingly rare. If those present, following Winckel, use them only once in 218 times, but few of us will live long enough to do a great deal of harm.

PROFESSOR T. D. FITCH said : Mr. President : I think a paper so commendatory of a measure as this, will perhaps lead many of the members of the profession to adopt it without proper precautions and without realizing the dangers which attend the use of the intra-uterine stem. I believe it is a very dangerous instrument to use. I am an advocate, as you all know, of pessaries, but I do think the intra-uterine stem a dangerous instrument, and that in less careful hands than Dr. Jackson's serious results will often follow. My own experience in the use of it has been limited, for the reason that I became alarmed from the bad reports of cases by Dr. Chambers himself, the inventor of this bifurcated instrument which Dr. Jackson has exhibited. If the same precautions are used that are advised by Dr. Jackson, I think, as a rule, it might be entirely harmless; no, I should hardly be able to say entirely harmless, or entirely free from danger, but I think the precautions which he has adopted have been very ingenious and would in the majority of cases prevent serious results from the intra-uterine stem. His use of the bougies preceding the use of the inelastic stem, accustoming the uterine mucous membrane, or the uterus itself, to the presence of a foreign body within its cavity, is very ingenious, and a thing I should never have thought of myself. Although I have tried these pessaries occasionally, my great difficulty has been to keep them in the uterus ; I might open them in any direction I pleased, spread the blades as widely as I pleased, and they would slip out—

they caused so much uterine contraction that they would be expelled from the uterine cavity into the vagina, and I have always been disappointed in the results from their use. For several years while I was in active practice I had adopted the treatment of Peasley for flexion and stenosis, whether caused by exudation or spasmodic contraction of the os internum; that is, by the use of his uterotome dividing the stricture at the internal os, and then gradually dilate the canal until I could introduce a No. 12 or 14 sound through the os internum. This was introduced every second day from one week to two weeks, until it ceased to be followed by pain and by hæmorrhage after its introduction, showing that the os internum had been thoroughly dilated and the incision had healed sufficiently so that no blood followed the use of the sound. After the sound was introduced I used a large glycerine tampon for the purpose of depletion and relief from irritation and to support the uterus; if it was an inversion it would hold the fundus up so as to assist in relieving the flexion to a certain extent at least, and preventing the occurrence of inflammation. I have treated a great many cases in this way and with entire satisfaction, and never had a case of acute inflammation of any kind occur as the result. I believe, however, that a majority of cases of flexion are attended with versions more or less. I don't believe that flexions occur so frequently as is generally supposed, unaccompanied with version; the uterus is tipped over more or less in connection with the flexion, and in connection with the treatment which I have suggested I have always corrected the version, and used the ordinary support or pessary to keep the uterus in its proper place, thereby relieving any contraction or pressure which would keep up

the flexion. I think the paper an admirable one, and the Doctor's precautions in the use of the instrument he has advised commendable.

DR. H. C. FEDER said : I would like to ask for information. In the report of these sixty cases of inversion and retroversion it has not been stated how many were accompanied by prolapsis, or what was the cause of flexion ; whether in married women getting up too soon after confinement, or whether from acute inflammation. The paper does not go into the facts and state whether the uterus is lightened and thereby goes back of itself to a normal position, nor does it inform us if this could be assisted by giving medicine internally. Much depends upon the state of the patients at the time of treatment,—whether they are in a healthy condition, or whether they have some specific blood disease in which medicine would assist in the treatment. And if the medicine has an alterative effect, how much benefit is received from the medicine and how much from the pessary.

PROFESSOR JACKSON said, in concluding the discussion : Mr. President: I feel that I ought to express my thanks for the courtesy with which this paper has been received. It is only a thirteen minute paper, and there are a great many things in the domain of medicine that are not in it, and a great many questions might be asked on subjects growing out of and connected with it which I could not answer if I were disposed to. The intention of the paper is simply to demonstrate the efficacy of a single remedy in correcting a single deformity. Questions as to whether the uterus was prolapsed, whether the patients had taken antibilious pills, or had cachexia, really do not enter into the consideration of the subject. I supposed that was

perfectly plain from the fact that no mention was made of anything beyond the mere condition of deformity. I am very glad so many excellent ideas have been added to it. The suggestion of Dr. Nelson as to the patient being able to withdraw the pessary is excellent, and is never omitted. I never introduce a pessary that I do not attach to it a silken cord by means of which the patient can withdraw it in case of necessity. As a rule, every patient should be able to withdraw any instrument placed in the genital passages; the regular attendant may not be at hand when needed, there may be an aversion on the part of the patient to calling in another physician, and she should have the proper means at her disposal. The remarks of Dr. Dudley as regards the distinction that should be made between pathological and physiological conditions resulting in flexion are quite proper, and I agree with him fully. We all know that the conditions preceding and accompanying these bent conditions of the uterus are very various; and in many cases no stem, incision, or other means will have a beneficial effect, although they may for a time cause the uterus to be straight. But the mere straightening is not always the main element of cure. When the uterus has been chronically fixed there will be a thinning of the side towards the angle, showing local failure of nutrition either as cause or effect of the bending. Straightening, therefore, is one element of cure in a uterus where there is insufficient nutrition, and I do not believe that any other means exclusive of this cures flexion. But it must not only be straightened, but its circulation must be fully restored, otherwise the organ will resume its bent condition. We cannot put a splint on the outside of the uterus, and the intra-uterine stem affords a means by which the uterus may be kept straight enough to allow of circulation on each side. The method of treating flexions by forcible and extensive dilatation

does more than dilate. It straightens, also. A bend may be just as acute in a large tube as in a small one, and mere stretching will not suffice, and its results usually not be permanent. Gradual dilatation is much more promising, and next to the method by the stem I would prefer it. I have only treated cases of flexion in which dysmenorrhœa was present, a symptom that interferes with the patient's health, and the dysmenorrhœa was usually cured or relieved. I do not think this such a wonderful success; only about two-thirds of the cases were cured, some were simply improved, and in some I do not know the result. Yet I think there is no other method that will do quite as well. The suggestions made in the discussion accounting for the safety and success of the treatment are, I think, correctly attributed to the preliminary measures—the slowness of the straightening and the promptness with which any tendency to harm could be met. The object was to accustom the uterus to its tenant, so that by and by it would accommodate a larger one, and in this way the uterus has been made to receive and tolerate the presence of an inflexible instrument. In one case a Chambers stem was retained twenty months, and I think if the patient had not returned and told me she was wearing it it would be there yet. It produced no unfavorable symptoms.

DR. A. R. SMALL reported

A CASE OF PISTOL-SHOT WOUND.

May 2d, 1886, he was called to see F. R., aged 23, who, a few minutes previously, had received a shot from a No. 32 pistol. Patient was suffering from shock, difficult breathing, and excessive pain in the left leg below the knee. The ball had struck the right eighth rib, about two inches external to the costal cartilage. Sensation was lost in the right leg below the knee. Motion was not impaired in the right leg, though the sensation was lost below the knee. The left leg was hy-

peræsthetic below the knee, and motion slightly impaired. A drainage-tube was inserted about two inches into the wound, and the wound dressed antiseptically. The patient complained of no pain except in left leg below the knee, where the pain was excessive. Morphia was given hypodermically in sufficient doses to control the pain. Nothing was allowed the patient the first twelve hours but ice, and occasionally water. About 10 P. M. there was evidence of internal hæmorrhage, and the patient seemed to be sinking. Milk was then given in small quantities frequently. The morning of the 3d he had rallied somewhat.

The urine was drawn by the catheter every eight hours, and contained blood. There was no expulsive force to the bladder. Respiration was normal after the first two hours.

On the afternoon of the 3d patient became delirious, and continued so, with occasional lucid intervals, until death, which occurred at 4.20 P. M. of May 4th.

Autopsy five hours after death. Rigor mortis well marked. Unfortunately, through a misunderstanding, the undertaker had preceded us and injected his preserving fluid, so that we were unable to determine exactly the amount of blood in the right pleural cavity. It must have been quite large, however, as the right lung was entirely collapsed. The ball made a clean round hole through the center of the eighth rib on the right side, about two inches from the costal cartilage, passed through the lower side of the right pleural cavity, without injuring the lung, passed through the diaphragm, right lobe of the liver, and superior portion of right kidney, and through the intervertebral foramen between the eleventh and twelfth dorsal vertebræ, on the right side of the spine, and lodged against the posterior surface of the body of the eleventh dorsal vertebra, just within the spinal cord, where it was so firmly

imbedded that it could not be removed without disarticulating the spine, which, for sufficient reasons, we did not do.

Though we found the right lung collapsed, respiration had been normal except the first two hours after the injury.

DR. ALFRED S. HOUGHTON read a paper on

THE DANGER IN SPECIALISM.

He said that specialties had greatly increased medical knowledge and skill, and had secured for many much reputation. Hence young practitioners grasp any excuse for becoming specialists. But man is not a machine, but a complicated organism, and disease is complex, one organ sympathizing with another; hence it is necessary to examine every portion of the body and treat all organs affected. Hence there is danger in a specialist limiting his sphere of action and usefulness to an unnecessary degree. Another danger is that specialists are apt to become egotistic, and give rise to utterances which they will afterward regret.

DOMESTIC CORRESPONDENCE.

HOT-WATER BATHS IN PUERPERAL PERITONITIS.

TO THE EDITORS OF THE *Chicago Medical Journal and Examiner*.

Gentlemen:

In February, 1874, Mrs. F. was attended by two physicians, for what was pronounced to be peritonitis, for three weeks, she growing worse steadily. Her condition at that time was as follows: High fever, feeble pulse, 130, tympanitis so pronounced that a regular arch was formed from the neck to the pubes, severe pain and inability to swallow even a tablespoonful of fluid; urine had to be drawn with catheter. In that state the prognosis was bad, and the physicians agreed that she could not live twenty-four hours longer.

I then commenced to immerse the patient in hot water raised to the temperature of 150°, and kept her in the bath at that degree of heat for an hour, and continued to do so three times a day for the first four days, then twice a day for the remainder of the week, after which I used the hip bath once a day.

When first put into the bath she was so weak and the tympanitis so pronounced, I could not keep her head from sinking under the water while the body floated. I had recourse to a heavy log surmounted by a pillow, on which I rested her chin, she being face down in order to facilitate the escape of gas *per anum*, which took place in about half

an hour after immersion. It was most offensive, and the room had to be disinfected and ventilated to prevent her from vomiting. The gas continued to escape in large quantity during each immersion. The benefit derived from the first bath was so marked that for the first time in three weeks she slept soundly for at least four hours, and on awakening could take some burned brandy. She gradually gained, and on the third day asked for the bedpan, under the impression that her bowels were about to be moved. On removing the pan I was surprised to find from a pint to a pint and a half of pure laudable pus instead of fæces. The discharge of pus continued for about a week, when it ceased entirely. She made an excellent recovery, no fistula being discoverable, and in the month of June traveled from Laramie City, Wyoming, to St. Louis. She is still living and enjoying tolerably good health.

Yours, etc.,

F. W. FITZ-GERALD, M. D.

318 Centre avenue.

July 14, 1886.

ABSTRACTS.

RECENT PROGRESS IN DERMATOLOGY.

Henry J. Reynolds, M. D., Professor of Dermatology in the College of Physicians and Surgeons, of Chicago, in his annual address as Chairman of the Committee on Dermatology in the Illinois State Medical Society, which convened at Bloomington, May 18, 1886, said:

"It is difficult if not impossible to determine in a year's time whether certain advances and discoveries in this department, as in other departments of medical science, are really such or are only so-called. It would not, however, be speaking in very flattering terms of the intelligence of the writers of the vast array of literature that is yearly presented to the medical profession, nor of those who eagerly peruse the same, were we to assume that even only a little in the direction of advancement had been presented. While, however, but comparatively little perhaps in this department has been presented during the past year, that can in every respect be regarded as real advancement, very many important clinical facts and experiences have nevertheless been presented, which, in addition to establishing new theories, serve the equally important purpose of tending to show the fallacy of old ones. Therefore, not presuming to present all that has appeared on record in this light during the past year, I shall ask the indulgence of the SOCIETY while I enumerate such as have come to my notice or been deemed worthy of mention, together with some personal remarks regarding the same, and leave the matter as regards the de-

termination of the truthfulness and value of the so-called advances for further investigation and your wise judgment to decide."

He then referred to the grouping together of several rare conditions by Duhring under the one general head of what he called dermatitis herpetiformis, and to certain other changes that had been recently made in the nomenclature of skin diseases.

He next, in his remarks on acne, spoke of the procedure recently proposed by Sherwell, of Brooklyn, for that disease, viz., the passing of sounds in the male urethra; and cited the cases more recently reported by Denslow, of St. Paul, as corroborative of the beneficial results of this supplementary measure in the treatment of acne. For the deep or indurated form of this disease he spoke of the method which he himself usually adopted, and which he believed was original with himself. He always lances those apparently papular indurations deeply, and invariably finds pus at the bottom, with a tendency to burrow still deeper, rather than come to the surface. He then, to prevent the wound from healing over on the surface—which it is otherwise sure to do, and leave the miniature subcutaneous abscess to go on as before, and remain indefinitely—passes a probe dipped in carbolic acid to the bottom and fills the whole incision from bottom to top with a small piece of absorbent cotton and leaves it to granulate from the bottom, which it will never fail to do, leaving only an almost invisible scar.

He referred to a suggestion made by Fox, of New York, as to the possible, if not probable, cure of leprosy, if the proper moral effect could be brought to bear upon those patients, stating that the moral effect was usually depressing in the extreme, banished as they usually are from society, and imprisoned for

life, as it were, with only the promise of a most miserable existence and a lingering death.

He referred briefly to the pigmentary syphilide and to a case reported by Dr. R. W. Taylor, of New York, and one by himself of this condition.

He then cited the treatment suggested by Bulkley for carbuncle without incision and poultice, viz: an ointment containing ergot and oxide of zinc locally, and sulphide of calcium and saline laxative with iron internally. He also spoke of the treatment so highly recommended by Dr. Hibberd, of Richmond, Ind., of applying with friction every three hours oleate of morphine.

He then, in speaking of urticaria, said that Lassar claimed to have reduced the frequency and cut short the duration of violent attacks of this disease by giving twenty-four grain doses of salicylate of sodium repeated every two hours until three doses have been taken.

In ringworm of the scalp Alder Smith recommended seven grains of chrysarobin to an ounce of chloroform, his theory being that the chloroform dissolved the sebaceous and fatty substances away, and favored thereby the penetration of the chrysarobin. A. J. Harrison, he said, for this affection, applies first a mixture of one-half a drachm of iodide of potassium to an ounce of liquor potassii. When this has penetrated he applies a solution composed of three grains of bichloride of mercury to the ounce of sweet spirits of nitre; his theory is that the liquor potassii softens the hair bulbs and facilitates the penetration of the iodide of potassium. Then when the mercury mixture is applied the biniodide is formed at the bottom of the follicle, a chemical change which he claims to be very beneficial.

The writer then referred to the use of chrysarobin inter-

nally, so highly recommended by Stocquart a short time ago for various skin diseases. For theoretical reasons and from experience he did not believe the drug had any special value as an internal remedy; and even the original advocate did not, of late, claim so much for it as formerly.

Brief reference was then made to the use of cocaine in dermatological practice. He said he had found it to be of some service to allay itching in certain cases of eczema and in pruritis ani, in strong solution. He had used it to relieve the pain incident to the removal of superfluous hairs by electrolysis, but the remedy was not sufficiently absorbed by the skin, as a rule, to be very beneficial.

BOOK REVIEWS.

1. THE STUDENT'S MANUAL OF VENEREAL DISEASES ;
being a Concise Description of Those Affections and of Their Treatment. By BERKELEY HILL, M. D., *Professor of Clinical Surgery in University College, London, etc.*; and ARTHUR COOPER, M. D., *Surgeon to the Westminster General Dispensary, etc.* Fourth edition, revised. Philadelphia: P. BLAKISTON, SON & CO. 1886.
2. A MANUAL OF DISEASES OF THE SKIN. By BALMANNO SQUIRE, M. B., *London, Surgeon to the British Hospital for Diseases of the Skin, etc.* Chicago: A. N. MARQUIS & Co., *Clark and Adams streets.* 1886.
3. CUTANEOUS MEMORANDA. By HENRY G. PIFFARD, A. M., M. D., *Clinical Professor of Dermatology, etc.* Third edition. New York: WILLIAM WOOD & CO. 1885.

4. *VENEREAL MEMORANDA; a Manual for the Student and Practitioner.* By P. A. MORROW, A. M., M. D., *Clinical Professor of Venereal Diseases, etc.* New York: WILLIAM WOOD & Co. 1885.

1. The fourth edition of the manual written by Messrs. Hill & Cooper, has all the merits of those which preceded. It is, like many of its fellows, defective in the important matters on which it does not and cannot touch ; but this is true of all hand-books concisely written. For example, the important subject of chronic prostatitis is here considered in four lines ; and, if the student who purchases the volume should be confronted with a case of this character and should desire to know how to make a differential diagnosis between chronic prostatitis of blennorrhagic origin, and one of the forms of the too common tubercular affection of the prostate, he would have to look elsewhere for his facts.

All said, however, the manual is an excellent guide to the student in the field which it concisely covers.

2. Squire's little volume comes to us with the musty aroma of a past generation of dermatologists ; and is an interesting souvenir for the collector of dermatological bric-a-brac of the day when Willan taught how to classify diseases of the skin by the external lesion. For example, under the heading "bullæ," we find discussed "pemphigus and rupia!" Can we trust our eyes ? Is this the day when at least two different forms of bullous exanthem have been shown to originate from ingestion of the iodine compounds alone ? Have we been sleeping like our friend Rip Van Winkle, and is this the rusty flint-lock that lay at our side when we went up the mountains twenty years ago ? This is not a laughing matter ; it is one of the most interesting

little souvenirs of the way dermatology was taught years ago that one can pick up for its price.

3. Piffard's work in its third edition, with all the author's odd opinions on "rheumides" and the like, is far in advance of the Englishman's miniature book. It is well known, and deserves the estimate that the young gentlemen place upon it when they study the cases in the clinic. It is, however, surpassed by Duhring's little manual.

4. Morrow's little companion to Piffard's manual is really well and concisely written, and bears evidence of having been thoughtfully prepared. It is epigrammatic in its terseness, accurate in its statements, and useful in many ways to the beginner; scarcely to others. There are objectionable features which may be pardoned in the general excellence of the book. For example, when we read, that "a prostitute is safer at night than in the morning, when contagious secretions have had time to accumulate," we feel like entering a protest against all such literature. Far better direct men how to light cigars in the vicinity of nitro-glycerine. Dr. Morrow retains the nomenclature which includes such terms as "ecthyma-form syphilide," "impetigo-form syphilide," etc. We have before this protested against describing one disease in terms of another. When the student reads of a "variola-form syphilide," he wonders what variola looks like; and, if advanced enough to know, he naturally asks, "in what stage of the evolution of the variolous papulo-vesiculo-pustule does it resemble the special syphiloderm here described?" better go back to the fathers of medicine, who called diseases of the skin by names which children could understand, such as "honey," "wolf," pig, etc.

COMPARATIVE ANATOMY AND PHYSIOLOGY. By F. JEFFREY BELL, M. A.; *Professor of Comparative Anatomy at King's College. 16mo, pp. 555, Illustrated. Philadelphia: LEA BROTHERS & CO. Chicago: A. C. McCLURG & CO.*

We have here an interesting treatise, the subjects being introduced in a somewhat original manner. The author first directs attention to the question of living matter, treating it comprehensively as relating to animals and plants, beginning with a definition of protoplasm, and following it up in its development into tissues and organs. He then, as he claims in his preface, "writes about organs rather than groups of animals," giving details of structure. This will prove to be a most valuable addition to the library of the student who desires to acquaint himself with the fundamental principles concerning the anatomical structure, development and physiology of the organs of lower animals, and for comparing the same parts of one animal with those of another. The writer's style is lucid and terse. It affords one pleasure to peruse the pages of this production.

F. C. S.

PRACTICAL ANATOMY. A MANUAL OF DISSECTIONS. By CHRISTOPHER HEATH, F. R. C. S.; *Professor of Clinical Surgery in the University College, London, etc., etc. Revised by RICKMAN J. GODLEE, M. S., London, F. R. C. S. Octavo vol., pp. 567, Illustrated. Philadelphia: P. BLAKISTON, SON & CO.*

Professor Heath's work has reached its sixth edition. It is a trifle enlarged over preceding editions, and has fourteen beautifully colored plates representing practical dissections,—the arteries and veins being stained red and blue, respectively. The wood-cuts throughout the entire book are quite satisfactory, showing the finer lines more distinctly than is usual

in works of this character. The reviewer mentions this fact because of its importance to the young student of medicine, to whom a correct picture often "talks clearer than words." The directions for making a dissection are explicit and to the point. The advice of the author as to the "method of study" should be heeded by all dissectors. The descriptions are accurate and the book fully sustains its past reputation as a practical working guide for the student. F. C. S.

BOOKS RECEIVED.

Reports of the Surgeon-General of the Navy. Six volumes, 1879, '80, '81, '82, '83, '84.

Hygienic and Medical Reports United States Navy 1879.

Traite Elementaire d'Anatomie Medicale du Systeme Nerveux. Par Ch. Tere. Paris: Bureaux du Progres Medical.

Our Penal Machinery and Its Victims. By John P. Altgeld. A. C. McClurg & Co., Chicago.

A Treatise on the Diseases of the Nervous System. By William A. Hammond, M. D. New York: D. Appleton & Co. Chicago: A. C. McClurg & Co.

Medicine of the Future. By Austin Flint (Senior), M. D., LL. D. New York: D. Appleton & Co. Chicago: A. C. McClurg & Co.

Analysis of the Urine. By Hoffman & Ultzmaun. New York: D. Appleton & Co. Chicago: A. C. McClurg & Co.

Bright's Disease and Allied Affections of the Kidneys. By Charles W. Purdy. Philadelphia: Lea Brothers & Co. Chicago: A. C. McClurg & Co.

Medical and Surgical Directory of the United States. R. L. Polk & Co.

PAMPHLETS RECEIVED.

Report of the Board of Managers of the Pennsylvania Hospital 1886.
Is Disease of the Uterine Appendages as Frequent as It Has Been Reported?

By Henry C. Coe, M. D.

The Influence of Sewerage and Water Supply on the Death Rate in Cities.
By Erwin F. Smith.

Report of a Case of Cesarean Operation, with Some Comments. By Edward
W. Jenks, M. D.

Typhoid Fever in Philadelphia. By Henry Leffmann, M. D.

Tetanus. By N. Senn, M. D.

Comments on Pasteur's Method of Treating Hydrophobia. By Charles W.
Dulles, M. D.

On the Necessity of Organization of the Medical Profession. By F. E.
Daniel, M. D.

On the Limitation of the Contagious Stage of Syphilis. By F. N. Otis,
M. D.

American Public-Health Association. Preliminary Circular.

The Present Status of Abdominal Surgery. By N. Senn, M. D.

Excerpta from the Biennial Report of the Board of Health of Louisiana.

*A Brief Synopsis of the Various Points Involved in the Coarse Examination
of the Brain and Spinal Cord.* By Francis X. Dercum, M. D.

Malarial Manifestations Due to Traumatism. By Henry C. Coe, M. D.,
M. R. C. S.

Esthetics of Medicine. By H. A. Cottell, M. D.

Enucleation with Transplantation and Reimplantation of Eyes. By Charles
H. May, M. D.

Marion County, Florida; an Ideal Winter Climate. By George Troup
Maxwell, M. D.

Report of a Case of Successful Transfusion in Typhoid Fever. By W.
S. Whitwell, A. M., M. D.